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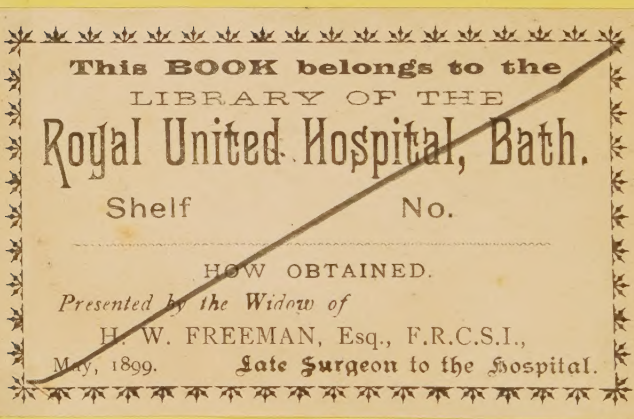

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BRAITHWAITE'S RETROSPECT.

VOL. LXXXVII. JANUARY—JUNE, 1883.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D.Lond.

FELLOW AND LATE VICE-PRESIDENT OF THE OBSTETRICAL SOCIETY OF LONDON.
LECTURER ON DISEASES OF WOMEN AND CHILDREN AT LEEDS SCHOOL OF MEDICINE.
SURGEON TO THE LEEDS HOSPITAL FOR WOMEN AND CHILDREN.

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Letters for the Editors to be addressed

DR. WM. BRAITHWAITE, Clarendon House, Clarendon Road, Leeds; *or*

DR. JAMES BRAITHWAITE, Clarendon Road, Leeds.

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AN ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, WITH OTHER SHORT ARTICLES FROM THE MEDICAL JOURNALS, SHOWING THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS DURING THE HALF-YEAR. ARRANGED ALPHABETICALLY.

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER OF THE BREAST.—Surgeons, as a rule, do not remove cancers of the breast. They persuade their patients that they do, and they almost persuade themselves. But there is always that little bit which they leave behind, and which they fondly hope will not grow, because it is such a little bit. I contend, therefore, for a return to the old plan of sweeping everything away and leaving a great hole, if you like. The operation will no longer be the bit of surgical tailoring that it has been, and many more persons will die from it. But many more also will be spared to live useful lives, and escape the horrors of a return tenfold more distressing than the original evil. I most strongly urge the invariable clearing out of the axillary glands along with the removal of the breast, the one operation being useless without the other. As you cannot tell whether the glands are affected or not till you see them in your hand, let them be *always* removed, and so increase the patient's chances of future immunity. (Mr. W. Mitchell Banks, p. 138.)

Amputation of the breast for fungating carcinoma may be undertaken without the use of the spray. In such cases, it is better to wash the wound thoroughly with chloride of zinc, and then to sprinkle it with salicylic acid and dress it with salicylic lint. The wound should not be closed by stitches. It may be necessary to employ compression to check subsequent hemorrhage; and for this purpose an ordinary bath-sponge, squeezed dry and enveloped in gutta-percha tissue, forms an elastic pad which is always well borne. The whole breast should always be removed, even when a few out-lying glands only are secondarily infected; but in cases where the disease has so far extended as to leave no margin of healthy tissue, it is better to leave it untouched. Cancer of the penis has the least, and cancer of the breast the greatest tendency to recurrence. (Prof. Nussbaum, Medical Record, Nov. 15, p. 443.)

Diphtheria and other Infectious Diseases.—*Eucalyptus Globulus* (*Blue Gum*) *as a Disinfectant.*—It is an acknowledged fact that in blue-gum we have a most perfect disinfectant; not an artificial one, but one of nature's own; one always at hand, for it will grow in temperate climates. The green leaves hung in a bedroom keep it sweet; leaves placed on a wound, steam inhaled from it, or its infusion drank, or injected into wounds, all answer equally well. Prof. Lister speaks highly of the eucalyptus oil for wounds, and it is also spoken highly of in rheumatism. It has not an unpleasant smell, and is tolerated by nearly all. My mode of procedure in infectious cases is very simple. I pour boiling water on blue gum-leaves, in a tub, jug, or chamber, which I place beside the bed, and change it every half hour. If only one child is ill in the room I improvise a tent over the bed, either by means of an open umbrella with a sheet above it, or by placing a sheet over the sides or ends of the bed, and enclose the patient. It is wonderful to see how soon the pain in the throat and the swelling disappear, and the fever also. Pain in the stomach is the first symptom most patients suffering from the malady experience, even before the throat is affected. At first I used a simple fever mixture; but I found it was not needed, as the skin acted more or less according to the amount of steam used. The patients were able to eat bread-and-butter, the throat not being sore because I had not burnt it, as is the barbarous treatment recommended by some. In diphtheria the throat is never very sore unless caustics are applied. In simple cases many remedies will answer; but if it once gets to the larynx and below it no remedy can touch it except steam. (Dr. J. Murray Gibbes, New Zealand, p. 131.)

Fever.—"*Milk Epidemic*" *at Aberdeen.*—It was found that over three hundred individuals had suffered from the disease, and that all the sufferers had been using milk from the same dairy. A sample of milk secured for examination when the epidemic was at its height was found to contain numerous micrococci, spores of fungi and spores which resembled those of *Bacillus anthracis*—the organism which is associated with splenic fever. When cultivated, the spores germinated, first into exceedingly delicate bacilli, and then into spore-bearing filaments. On inoculating rats with the milk containing the spores, death followed in from eighteen to twenty-four hours. The tissues of the rats, especially in the region of the neck, were infiltrated with bacilli, which, on cultivation, developed into spore-bearing filaments. Inoculation proved both bacilli and spores to be as virulent as the original spores found in the milk. Confirmatory evidence of the relation of the bacillus

to the disease, and of the disease to the bacillus, was obtained by the examination of pus from an abscess over the angle of the jaw of one of the sufferers. This pus contained spores and bacilli similar to those found in, or developed from, the milk. Rats inoculated with a minimal quantity of the pus suffered and died in the same way as the rats infected with the milk and the milk-cultivations. Further investigations proved that the organisms had been added to the milk along with water. The water used at the dairy previously to the epidemic passed through a large concrete cistern (provided with a rough loose wooden cover) placed in the corner of the large byre immediately over the heads of several cows. The spores reached the byre along with the steamed hay used for food, and from the byre they had easy access into the cistern; how they reached the tank in which the hay was steamed has not yet been discovered. (Dr. J. Cossar Ewart, p. 122.)

Rheumatism.—*Acute.—External Application of Salicylate of Soda.*—On Oct. 20th, 1880, I was called to Mrs. S——, a strong healthy woman, aged forty. I found her with all the symptoms of an ordinary attack of acute rheumatism, the joints more particularly affected being the right knee and ankle. Temperature 103.5°; pulse 130. Heart sounds clear. This was the first attack. She was given twenty grains of salicylate of soda with five minims of tincture of digitalis every four hours.—21st: Right wrist swollen and tender, also left knee; right knee and ankle easier. Pulse 110; temperature 102°. I ordered a solution of salicylate of soda in water, twenty grains to the ounce, to be applied every half hour to the affected joints.—22nd: I found my patient very much better. She informed me that in less than half an hour after the first application the pain began to subside, and now she scarcely felt any. Pulse 100; temperature 100·5°. The same treatment was continued, and she made a rapid recovery. So pleased was she with the lotion that she begged I would leave her the prescription. In other five cases the symptoms were well marked, and very great relief always followed the use of the lotion. I did not at all times apply the solution to all the joints; when I did relief followed quickly, sometimes in a few minutes, sometimes a little longer. Those joints to which I did not apply it were not relieved like the others. (Mr. E. A. Whiteley, Duffield, Lancet, March 10, p. 436.)

Chronic Rheumatism.—Powdered Capsicum.—Powder of capsicum, two drachms to the ounce of prepared lard, rendered elegant by the addition of one of the essential oils, is a seldom failing local application in subacute and chronic forms of rheumatism. It must be rubbed over the affected

part for ten minutes by a gloved hand, and the application of dry heat intensifies the rubefacient action, which continues for a considerable time afterwards, being even somewhat revived by heat or walking. It is to be used freely night and morning, or at bedtime only (in lumbago, for example, the first thorough application often gives marked relief), according to the effect produced on the sensibility of the patient, and on his disease. (Dr. A. D. Macdonald, Liverpool, British Med. Journal, Feb. 10, p. 251.)

Scarlatina Maligna.—*Sulphurous Acid and Steel.* — The moment the throat begins to become affected, I administer to a child of five or six years of age 10 minims of the sulphurous acid with a small quantity of glycerine in water every two hours, and I direct the sulphurous acid spray (℥ ij.— ℥ iv. to the ounce of water, according to circumstances) to be applied every three hours to the fauces—about twenty squeezes; and when that can't be done, to hold the instrument about six inches from the mouth, and use it for a few minutes at a time. The acid solution must be recently prepared, as when it is kept for some time in water it takes up an atom of oxygen and becomes sulphuric acid. It is of some importance to bear this in mind, as the efficiency of the acid treatment depends entirely upon its composition. At the same time I administer a mixture containing from 3 to 5 grains of chlorate of potash with 7 to 10 minims of the tinct. ferri perchlor. in glycerine and water, more or less according to age, every four hours. I further direct a strong solution of permanganate of potash (℥ ij. or more to 6 ounces of water) to be held in readiness for laving the lips and mouth several times in the day to arrest the formation of the dark sordes which collect about these parts, some of which should be swallowed, if possible, each time the lotion is applied, gargling being out of the question in young children. Sulphur should also be burned in the sick-chamber three times a day at least, by placing flour of sulphur upon red-hot cinders on a shovel, and walking about the room with it, thus diffusing the sulphurous acid vapour through the apartment, until the atmosphere becomes a little unpleasant to breathe. In cases of an adynamic type, when medicine can't be swallowed, the spray and sulphurous acid vapour must be entirely relied upon. It is only in the very worst cases that I employ all the above remedies. As a rule, the sulphurous acid alone is quite sufficient to combat most cases of scarlatina anginosa; but when there are white patches at the back of the throat, I invariably administer steel and chlorate of potash as well, and both patients and their friends have frequently remarked to me, under such circumstances,

that an improvement took place after the first dose of the medicine had been swallowed. Of course, while vaunting the above remedies so highly, I do not forget that other details have to be attended to, such as the importance of fresh air and its free circulation, when that can be attained, the constant presence of disinfectants in the room, such as carbolic acid, chloride of lime, Condy's fluid, &c., the proper disposal of all discharges, the burning of rags containing sputum, &c., but among the poorer classes of society, where, as a rule, the worst cases are to be met with, details are seldom carried out, and one can only just do the best he can under the circumstances. As regards diet, milk, beef-tea, and wine should be frequently administered *when possible*, and sucking ice, which, as a rule, can be well borne. In the majority of these cases, however, little nourishment can be taken until all imminent danger is over. (Dr. K. N. Macdonald, p. 135.)

Three years and a half ago, I had about thirty cases of scarlet fever among the children of the soldiers quartered at Kingston, Surrey. All the cases were treated with diluted sulphurous acid internally. Externally, they were sponged over daily with warm water; as the feverish symptoms and rash declined, their bodies were rubbed over with sulphur ointment, to prevent the scurf particles from flying about the room, as well as to destroy their vitality. Sulphur was also burned frequently during the day, under the bed and in different parts of the room. All the cases did well. Two or three cases of dropsy occurred, from carelessness in letting the children play about in draughts too soon. I have treated many cases of scarlet fever, during thirty years, in various ways; but no series of cases ever did so well, or gave less cause for anxiety, than these. (Mr. C. M. Jessop, Preston, British Med. Journal, Feb. 24, p. 356.)

Typhoid Fever.—*Destruction of Germs by Spongy Iron Filtration of Drinking Water.*—Typhoid fever is popularly supposed to be due to bad smells and sewer gas, but it is really due to the germs contained in these; and in reference to these there are two noteworthy points. One is, as shown by Carpenter, that if sewage be poured through Italian rye-grass the spongioles destroy all germs, and leave the resulting fluid so far harmless. The other is, that although germs will pass through the finest filters, filtration through spongy iron entirely destroys them. The well water of Antwerp—now rapidly becoming the greatest emporium of Europe—is brackish, and unfit to drink; accordingly the inhabitants have recently got up fine waterworks in which they apply this spongy iron filtration (the invention of Prof. Gustav Bischof) on

a large scale to the water obtained from the river Nette. Dr. Frankland has visited the Antwerp waterworks at Waelheim, about fifteen miles above that city, and reported on the results of his inquiry. He attaches especial value to the fact that spongy iron filtration 'is absolutely fatal to bacteria and their germs,' and he considers it would be 'an invaluable boon to the metropolis if all water supplied from the Thames and Lea were submitted to this treatment in default of a new supply from unimpeachable sources.' (Dr. F. J. B. Quinlan, p. 107.)

Zymotic Disease.—*Benzoate of Ammonia.*—A year and a half ago, I published a note in a contemporary journal in reference to the antiseptic treatment of what are called the zymotic diseases (I trust we shall soon be naming them the protistozooic diseases), mentioning the administration of carbolic acid in scarlatina; but I have since then, with more advantage, used the benzoate of ammonia as my germitox (a word meaning germ-poisoner better expresses the idea of the various degrees of antiseptic action than a term meaning germ-slayer). In doses of fifteen grains every three or four hours to an adult, and proportionately for children, it is an efficient treatment in scarlatina anginosa, and milder forms, combined or not with liquor ammoniæ acetatis. I have been informed, by a friend, of its uncombined exhibition in an apparently hopeless case of scarlatina malígna, where recovery followed. (Dr. A. D. Macdonald, British Med. Journal, Feb. 10, p. 251.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Alcohol as a Sedative.—Alcohol in its various forms, as in popular use, I maintain to be a drug possessing purely sedative properties, and in its method of action is allied to opium. It primarily affects the sympathetic nerves, then the vagus, and finally the cerebro-spinal system. When the nerves become subject to a full non-toxic dose, the pupil contracts; but soon after a fatal dose has been taken, the pupil dilates some time before death. The action of alcohol upon the heart and blood vessels, through the vagus and sympathetic system, is also analogous to that of opium. Primarily it attacks the blood-vessels through the sympathetic nerves, diminishing their tonicity—thus relieving the heart from blood pressure—so that the initial signs of its action may be a temporary acceleration of the pulse, as well as an increase of its volume, simulating stimulation; but if the dose is sufficiently increased, then the heart is also affected, and the pulse becomes reduced in rate. Again, by alcohol, the solid constituents of the liquids secreted are diminished, and the

normal quantity of carbonic acid exhaled by the lungs is reduced; and in corroboration of these ascertained data there is to be observed a fall of temperature. All these signs point to a purely sedative result—retardation of life, no acceleration or stimulation. The simulated signs of stimulation by alcohol arise from the primary affinity that certain doses of alcohol have for certain nerve structures, and a misinterpretation of the signs of its primary action has engendered the belief that true stimulation is gained; and though sometimes this mistake in practice may do no harm, yet when true stimulation is required its administration would be a serious error. Its primary effect cannot be a safe substitute when stimulation is urgently demanded and requires to be continued. It may be argued that contraction of the pupil, when influenced by alcohol, may be brought about by stimulation of the circular muscular fibre of the iris, and not by paralysis of the radiating muscles; but the only explanation admissible regarding the mechanism of the increased volume of the arteries when influenced by alcohol, enables us to check our deduction regarding the mechanism of the action of the iris under its influence. If this drug could stimulate, the diameter of the blood-vessels would be lessened from contraction of their circular muscular coat. Again, if alcohol could stimulate, then its primary affinity for structures specially under the control of the sympathetic would cause this stimulating property to influence first the radiating fibres, and dilatation would be the first alteration observed in the pupil during its first stage of action. (Mr. H. Owen Thomas, p. 156.)

CHOREA.—*Large Doses of Arsenic.*—A little girl, ten years old, owed her recovery from chorea to the administration of arsenic. We had to give the remedy freely before the disorder gave way. The case was one of subacute general chorea, of moderate severity, occurring in a weakly, nervous girl. We began with five minims of Fowler's solution of arsenious acid, thrice daily, in an ounce of water. In three days, the dose was increased to ten minims; in three days more, to fifteen; in three days more, to twenty; and so on until she was taking thirty-five minims of the solution thrice daily. When this last dose was reached, the choreic movements, which before had been gradually subsiding, entirely ceased; and a little vomiting warned us that we had reached the first and most usual physiological action of our remedy. We then withdrew the drug for two days; after that time we gave it again, in fifteen-minim doses, for a few days more, when we gave it up altogether, and the child remained well. I have treated many cases of chorea in this way with similar success. The

dose of liquor arsenicalis in chorea, as laid down in text-books, is too small. Chorea is often an obstinate affection. The more chronic cases frequently pass from doctor to doctor, and go through long courses of medicaments, without benefit. Arsenic, freely and properly given, rarely fails. If a case of chorea come to you, and you learn that arsenic has been given and has failed, give it again, in large doses. You may cautiously increase the dose of liquor arsenicalis, far beyond the limits of the text-books, with the best results in chorea; in this way, you may usually cure cases which smaller doses of the remedy would not affect. (Dr. J. Sawyer, British Med. Journal, Dec. 23, p. 1240.)

Hysteria.—*Cauterisation of the Clitoris.*—The late Prof. Friedreich shortly before his death had prepared a paper which has since been published on this subject. In many cases of obstinate and severe hysterical affections he found that cauterisation of the clitoris by nitrate of silver had the most beneficial effects. The cauterisation must be severe, as slight superficial cauterisation tends rather to aggravate the disease. The pain is at first severe, and during it the patient must remain in bed. Amongst the cases which he gives as cured with extreme rapidity by this method are—one of paraplegia, which had lasted for a year and a half; hysterical aphonia, lasting for two years; glossoplegia, lasting for four months; tonic spasm of the spinal accessory, lasting for seven months; and several cases of general severe hysterical convulsions. (Practitioner, March, p. 214.)

Hysterical Vomiting.—There is no doubt that in most cases of hysterical vomiting, it is the stomach that rejects the food. But it is obvious that in an undetermined minority of cases of such vomiting, of which my case is an example, it is the œsophagus rather than the stomach that is in fault, and if, in such cases, the irritability or spasm of the gullet can only be overcome, and the food swallowed be allowed to reach its destination, the vomiting will cease. If one has reason to suspect the latter condition to be the cause of his patient's symptoms, it is fortunately easy to put the question beyond doubt by having recourse to the œsophagus-tube or stomach-pump; and, if the answer be in the affirmative, to cure the patient of her malady by the repeated use of the instrument and artificial feeding. There is reason, however, to hope that a single introduction may suffice to effect a more or less permanent cure. (Dr. J. S. Bristowe, Ibid, p. 174.)

Insomnia.—*Croton-Chloral Hydrate.*—On the recommendation of Dr. Collier, of Gorleston, I tried croton chloral-hydrate in doses of from five to ten grains, combined with chloral-

hydrate in thirty-grain doses; and, on account of the cardiac debility, I added drachm-doses of spirit of ether. This mixture produced sleep more effectually than anything else I have prescribed. (Dr. J. Ryley, Yarmouth, British Med. Journal, Dec. 16, p. 1238.)

Tannate of Cannabin.—Dr. Fronmüller of Fürth, through the help of Merck of Darmstadt, has obtained a preparation of hemp, the tannate of cannabin, which he finds reliable, effective, and easy to administer. The dose of the tannate ranges between 2 and 10 grains, the most frequent dose being 5 grains. The drug was used fifty-seven times in hospital and six times in private practice. From the sixty-three trials thus reported (in various diseases, the common symptom being insomnia), Dr. Fronmüller concludes that tannate of cannabin is a very useful hypnotic, powerful without being dangerous, and one which does not disturb the secretions or leave unpleasant toxic after-effects, if given in proper dose. (London Med. Record, Nov. 15, p. 453.)

MANIA.—*Hyoscyamine.*—The following formula for its preparation was brought into use, and has been found to meet every requirement as to strength, but lacks stability: Hyoscyamine (Merck's crystalline), four grains; glycerine, distilled water, of each half an ounce; carbolic acid, two minims; dissolve without heat. Dose, four to eight minims, given hypodermically. It is very important to make the solution without heat, as heat renders the alkaloid nearly inert. Hyoscyamine is a most unstable alkaloid, and soon decomposes; so that the strength of any solution yet devised cannot be depended on for more than a month after its preparation. In one case related, the patient, from gloomy and silent depression, gradually passed into a state of noisy and destructive mania, shouting incoherently, throwing himself off his bed, and dashing himself about, till he was much bruised, in spite of constant care on the part of the attendants. His restlessness was incessant, and he passed several days in succession without sleep. The use of hyoscyamine was begun in this case by the hypodermic injection of four minims (1-30th of a grain) of the solution mentioned above. This was continued twice daily for several days, without any marked effect except that of moderately dilating the pupils. The drug was then omitted for twenty-four hours, at the end of which time his pupils had contracted to their usual size. As such a dose was not sufficient to control or moderate the restless violence of this patient, it was increased to eight minims (1-15th of a grain), given hypodermically at 6 p.m. In fifteen minutes, his pupils began to dilate, and, becoming quieter, he lay down

in bed. Just before this, he had been jumping about the ward, resenting and resisting all persuasion to remain quiet or lie down. At 10 p.m. he was sound asleep, his pupils widely dilated. He could, however, be easily roused, but went to sleep again on being let alone, and slept for seven hours, and when he awoke his pupils had begun to contract again. He remained during that day quieter and more easily managed than he had been for a long time. The effect of the drug passed off in about twenty-four hours. We had now got the measure of this patient's power of resistance to the drug, and were thus able to regulate the dose in accordance with his seeming requirements. Often, from a restless, noisy, and destructive condition, he passed, while under the influence of the alkaloid, to a quiet and tractable state, easily managed by day, and obtaining several hours' sleep by night. Indeed, the soothing influence of the drug sometimes lasted for days; but at other times the nervous commotion seemed to get the better of the drug in about twelve hours, in which case an injection of eight minims (1-15th of a grain) was given, night and morning, with the effect of procuring quiet and comfort both to himself and those about him. His excitement came on in periods, and during their continuance quiet and rest were secured by the use of hyoscyamine, without apparent injury to the patient. The drug was omitted whenever the excitement was only moderate; sometimes he would go for days without it, and the dose was occasionally reduced to four minims, when that dose seemed sufficient to mitigate his violence. In this case, the effect of the alkaloid could be depended on and foretold almost with precision, if the drug were given by the skin; while, if administered by the mouth, there was an uncertainty both as to period of onset, duration, and degree of effect. It had, however, no curative power beyond the calming and soothing influence, which no doubt exerted a conservative effect on the patient's strength. As the excitement passed off, calmed and soothed by the drug, sleep seemed naturally to follow; he was disinclined for further effort, and, as in a child tired out with its own boisterous play, mind and body sank to rest. (Dr. Thomas Brown, Staff-Surgeon R.N., p. 142.)

Poisoning. — *Apomorphia as an Emetic.* — Those liable to be called to cases of poisoning are always glad to have an agent handy which, not in itself lowering, will produce prompt emesis, especially in those cases where the jaws are rigidly clenched and the stomach-pump absent or inadmissible. This agent I am sure we have in apomorphia, an alkaloid which Dr. Murrell has brought before the profession. Though

a derivative of morphia, it has no narcotic effects in the doses required to cause emesis. Dr. Murrell recommends it to be kept in a solution of 1 in 50 strength, and to be given subcutaneously in doses of from $3\frac{1}{2}$ to 10 minims ($\frac{1}{15}$ to $\frac{1}{3}$ grain). Emesis occurs in from two to five minutes, the contents of the stomach being usually voided in one rush without previous nausea, but with violent and visible muscular action of the stomach walls. (Dr. Amand Routh, p. 158.)

Opium Poisoning.—Nitrite of Amyl.—Two cases are recorded by Dr. Turner, in the St. Louis Courier of Med., of recovery from opium poisoning under the use of nitrite of amyl. Both cases were apparently bad ones, one of them being a child of six months, who had got half a grain of morphia by mistake. The inhalation was given very carefully, a few inspirations of the vapour at a time. The effect was apparent from the first, in the way of improvement of the pulse and respiration. After some hours the treatment was discontinued. (Glasgow Med. Journal, Jan. p. 75.)

Traumatic Tetanus.—Prof. Humphry has seen a great many cases of traumatic tetanus, and has arrived at certain conclusions respecting it. 1. That it is a passing malady depending, probably, like small-pox, scarlet fever, and others, upon some blood-poisoning; like them, it is amenable to no known curative treatment, but runs a certain course and subsides, provided it does not kill the patient in the meantime; that, like them, it is fatal in proportion to the acuteness and severity of its onset. Of the rapidly progressing cases very few survive, whereas in those which come on more tardily, and more particularly in those in which the patient continues to be able to take food, a favourable result may by good management not unfrequently be obtained. 2. That the disease is attended with much wasting and exhaustion, sometimes with high temperature, and that the most important feature in treatment is to give nourishment, which must generally be in a fluid form—milk, eggs, beef-tea, wine, &c.—as much as the patient can take. While food can be swallowed there is hope. It must be urged upon the patient, who is often reluctant to take it; and no other treatment should be allowed to interfere with this. Sedatives, such as morphia hypodermically injected, tobacco, and others, come in as adjuncts in the more severe cases; but are better abstained from unless the case is severe. Reliance should be placed solely on the feeding, with attention to the bowels; it is commonly necessary to give aperients. The worst case he had known to recover was that of an infant in whom the removal of a considerable tumour from the back

of the neck was followed by tetanus. The spasms were so frequent and so severe, that on several occasions the child was thought to be dead; and in this case, as in others he had observed, the seizures were most severe after a very prolonged interval, as after sleep. Still the child swallowed milk. The milk was sedulously given, no other treatment being allowed, except subjecting the child to the fumes of tobacco smoked by persons near it, which was thought to do some good. In other cases he had found tobacco, smoked or administered in a mild form, to have a soothing effect. He had kept patients persistently under the influence of chloroform without any benefit. The maintenance of strength by nourishment, so as to enable the patient to tide over the attack, is the great thing to be aimed at. (Prof. Humphry, Cambridge, Practitioner, Nov. p. 370.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Anæmia and Chlorosis.—In cases of anæmia and chlorosis a murmur is sometimes heard exactly in the site of that indicating mitral regurgitation. Assuming that in these cases there is a veritable regurgitation, how is such brought about? The mechanism for the closure of the left auriculo-ventricular orifice does not reside in the valve curtains alone; the surrounding muscles of the ventricle have an active share, not merely in floating up the valve curtains, but in reducing the size of the aperture which these valve curtains have to close. It is not that the orifice is dilated, but that it is *insufficiently contracted*, the aid of the muscles of the wall of the ventricle which normally produce such contraction being lost. It is important in regard to treatment to differentiate mitral regurgitation due to disease of the valves from that due to adynamia of ventricle, supposing a systolic apical murmur to be manifest in a markedly anæmic subject. The two signs I would most rely on as pointing to an anæmic causation of the murmur are: (1) an absence of notable cardiac dilatation; (2) a heightened tension in the systemic arteries. I have never known in these cases any marked improvement follow the administration of the usual cardiac tonics, such as digitalis and iron. In the cases attended with hemorrhage it is, of course, of the first importance to arrest this at its source. Rest and the administration of assimilable food are no less important indications. In this connexion I may call attention to the great value I have observed to attach to *supplementary alimentation by the rectum* in such cases. (Dr. A. Ernest Sansom, Lancet, Feb. 3, p. 184.)

Anœmia of Young Females. — Syrup of the protochloride of iron, in drachm doses thrice daily, diluted with water, recommended, I believe, by Prof. Fraser of Edinburgh, I have seen to be of great benefit, particularly in the anæmia of young females. It appears to me to take rank with, if not, all things considered, over, the famous Bland's pill. (Dr. A. D. Macdonald, British Med. Journal, Feb. 10, p. 251.)

Angina Pectoris.—*Nitrite of Sodium.*—Recollecting what I had some time ago thought as to the action of nitrite of amyl in angina pectoris being for the most part due to the nitrous acid, I resolved to make a trial of nitrite of sodium, which, if it were followed by satisfactory results, would offer therapeutical proof of the correctness of my supposition, and at the same time furnish, in addition to nitrite of amyl and nitro-glycerine, for the treatment of a distressing disease, a valuable remedy, inasmuch as it contained the nitrous acid in its simplest possible combination, uncomplicated by the presence of a base possessing an undesirable or disagreeable action. I therefore gave my patient the following prescription: ℞. Sodii nitritis, oz. ½; aquæ ad fl. ozs. xii. Solve. Sig. Dose, one or two teaspoonfuls. As I did not feel assured that the nitrite of sodium would prove serviceable, I asked him also to procure some nitrite of amyl, but in bulk, and to inhale five or six drops of it, as I was under the impression the quantity in each capsule was not sufficient to produce the desired effect. The expense, too, of the capsules, where so many were required, formed a consideration. The nitrite of amyl was only to be resorted to in the event of the nitrite of sodium proving inefficient. The patient returned with an extremely favourable account of the benefit he had derived from the use of the nitrite of sodium. He had taken, as I had suggested, a dose a few minutes before rising in the morning, and the result was that he was enabled to get up, dress, breakfast, and walk to the printing-office, without experiencing the slightest pain, and without requiring more of the nitrite until after dinner. He tried to do without it one morning, but the pain at once came on when he commenced to dress himself. After dinner he took it only when he felt the pain was coming on, and if he took it promptly it caused complete cessation of the pain in from one to two minutes. Nitrite of amyl never completely abolished the pain, not even when he inhaled from ten to twenty drops of it. It is possible, however, that the preparation he obtained in bulk was not good, for even one of the three-minim capsules which he formerly used had a stronger effect than twenty drops of it, although in no instance had the nitrite of amyl given him that complete freedom from pain which the

nitrite of sodium did. Another important difference was that the nitrite in the dose prescribed caused no perceptible throbbing in any part of the body, and certainly no headache. Apart from its effect on the pain, it seemed, he added, to have no more action than so much water. If he required to take it in the street, he could do so without having to stop for a few minutes afterwards and without feeling in the least giddy. In addition to taking it in the morning and after dinner, he took a dose always before leaving for home at night, and another at bedtime. He still continued to waken once or twice during the night with the pain, which a dose of the nitrite of sodium at once arrested. He felt very greatly pleased with his new remedy, for not only was it preventing or relieving the pain, but his general health was improving under its use (perhaps because it kept him free from frequent, almost intolerable, pains), and he felt fitter for his work than he had done for nearly two years previously. The preventive action of the nitrite of sodium is exercised over a much longer period of time than that of the nitrite of amyl. It is quite clear that the active remedial part of nitrite of amyl, nitroglycerine, and nitrite of sodium is the nitrous acid. The suggestion of this is not new, nor is there any novelty in its practical application, for, based on the analogy of the physiological action of nitrite of potassium to that of nitrite of amyl, Weir Mitchell, of America, was the first to recommend and try the use of a simple alkaline nitrite in the treatment of epilepsy, and later, in this country, Law, Saundby, and Ralfe, have recorded cases in which they have employed it with varying success. But as the pre-eminent therapeutical action of the nitrite of amyl rests with its use in the treatment of angina pectoris, it is particularly in this disease, against which all other remedies seem to be powerless, that we would expect to obtain the most valuable field for the employment of some simple combination of nitrous acid. The nitrites, on account of their remarkable physiological action, will, I venture to anticipate, in the form of their simple and more safely administrable compounds, soon have a wide and important application in the treatment of various forms of disease. (Dr. Matthew Hay, p. 160.)

Application of Sculptor's Clay.—Having perused Dr. Sokoloff's article (Medical Record, April 1882), Dr. Masalitinoff resolved to try the same method in a severe case of angina pectoris of four years' standing, in a patient aged 22, suffering from old postrheumatic insufficiency of the mitral and aortic valves. Of late, the anginal paroxysms appeared two or three times weekly, always at night, returning from two to four times, lasting from fifteen to sixty minutes, and preventing the

patient from sleeping. Quinine, amyl-nitrite, arsenic, valerian, &c., entirely failed to relieve the agonising pain. Dr. Masalitinoff ordered the application of cakes of moist sculptor's clay to the cardiac region twice a day. From the very first application, the patient felt greatly relieved, slept soundly through the night, and on the next day no traces of pain remained. At the end of three weeks, the patient remained entirely free from any cardiac pain; he was now scarcely recognisable; he became strong, cheerful, and gained in weight. The author used 'home-made' sculptor's clay in this case. He took finely ground plaster-of-Paris, and moistened it with water, thus making a soft paste, which he spread on a piece of linen and applied to the painful region. (London Medical Record, Nov. 15, p. 452.)

Dropsy.—*Concentrated Solution of Saline Cathartics.*—A concentrated solution of a saline cathartic ought to prove of considerable service in certain cases of dropsy, where, owing to the great accumulation of transuded serum in vital parts and elsewhere, there is imminent danger to life and an urgent need for an immediate and active removal of a portion of the transuded fluid. In such cases the value of saline and other active cathartics has long been appreciated, but I am not aware that use has been made of the more powerful action of a concentrated saline cathartic. It removes the dropsical fluid by two channels: by the intestines and by the kidneys. No other purgative has this double action. It is questionable, also, if any other purgative acts so rapidly in reducing the fluids of the blood. This is of the greatest importance in certain critical cases of dropsy. It is almost perfectly certain that no other purgative excites intestinal secretion so powerfully, and at the same time produces so little irritation of the intestinal mucous membrane and so little disturbance of the body generally. This is an additional recommendation for the employment of the concentrated saline. The diluted salt, the form in which it is always given, has practically, in so far as it affects dropsical fluids, the action only of a diuretic. Based on these considerations, I have made several trials of the concentrated salt in suitable cases of dropsy, and in most of them with very satisfactory results. In a boy aged 10, I prescribed three-quarters of an ounce of sulphate of magnesia dissolved in two tablespoonfuls of water, no water to be given afterwards. The result exceeded my expectation. When I called next evening, the patient was lying quietly sleeping in his bed. The anasarca was greatly diminished, and the dyspnœa had almost entirely gone, and his breathing was much slower. The pulse was also less rapid, and the pained, anxious expression of his face had vanished. His mother

told me that she had given him the salt as I had directed, and that in less than an hour afterwards the purgative action of the salt manifested itself, and there were repeated evacuations in the course of the next few hours; on each occasion the water seemed to 'gush' from him, and he passed an unusually large quantity of urine. (Dr. Matthew Hay, p. 181.)

Epistaxis.—*Hot Water Irrigation.*—M. Auquier mentions a case in which he was called to a young man of twenty who had been suffering for three hours from violent epistaxis. The patient had been subject to such attacks from infancy. M. Auquier tried in vain to stop the bleeding by means of cold water, plugging the nares, mustard plasters, &c. At last he irrigated the nose with very hot water, with instant success. During the next night and day the friends of the youth were able by this means to stop at the outset several fresh outbreaks. The author thinks the hot water acted by producing a reflex contraction of the bleeding vessels; and not by encouraging the flow and so causing depletion of the superficial vessels, as has been supposed, in reference to the stoppage of uterine hemorrhage. (Practitioner, Feb. p. 184.)

Ligature of Arteries.—Of late years a practice, which appears to me to be fraught with mischief, has come into fashion in the operation for ligature of important arteries in their continuity. I mean this: that after the neighbourhood of the vessel has been reached by a few preliminary incisions, it has become the custom to lay aside the knife and to seek for and expose the artery by tearing through the investing tissues with the end of a director or some similar instrument. The motive for this practice, which, I think, has been derived from the Continent, is obvious enough, and similar to that which formerly suggested the use of a silver blade. It is the fear of injuring the artery itself, or of some branch near to its origin, by the knife, when dissecting closely upon it, and so of having to deal at a critical stage of the operation with troublesome hemorrhage. But if the knife be used with fair knowledge and ordinary skill, the artery itself, or even a branch of it, ought not to be in danger; and assuredly the risk of yet more serious injury in another way from forcibly tearing through the textures with a blunt instrument is imminent. Indeed, it is almost impossible to avoid bruising the artery or vein, or to make a clean and satisfactory isolation of the artery by this plan. It gets exposed, indeed, and a ligature can be passed around it; but the surface of the vessel, when thus separated from its sheath, does not appear smooth as it ought to do, and already before the operation is finished there is often evidence of some ex-

travasation of blood upon the surface. The less complete the isolation of the artery at the particular spot where the ligature is to be applied, so much the greater is the risk of injuring the vein by the needle. Herein, I think, lies the chief cause of the dangerous accident of transfixing the vein. If all the work of exposure and isolation of the artery is to be done with a director or some such instrument, if no cutting is to be allowed and only tearing practised, what is to become of the rule, so universally insisted on, of opening the sheath with the least possible amount of disturbance, and of separating it from the artery only to an extent sufficient to allow the needle to pass? How is the sheath to be opened and treated, according to the recognised principles of surgery, with a director? While it has happened to me, in more than a single instance, to see the artery injured both on the dead and living by the abuse of a director, I have never seen the artery itself cut by the knife, even when it has been used too freely by awkward hands. I believe a surgeon who is fairly up to his work will tie an artery more safely by exposing it with the knife than with such an instrument as a director, and there is probably no operation in surgery in which the result so depends on the manner of performing it. It is not enough to tie the vessel and nothing but the vessel. It is of supreme importance to tie the vessel with the least possible amount of disturbance or injury. And, while writing on this subject, I may add that, with this object in view, perhaps it is more convenient to carry the needle round the artery, without the ligature, and to pass the thread through it afterward. (Mr. W. S. Savory, Lancet, Feb. 3, p. 186.)

Catgut Ligatures.—The catgut ligature is an immense improvement on the old fashioned silk one. It is bland and innocuous in its character, and capable of absorption by the tissues without injury to the patient. In the deligation of arterial trunks in their continuity in the operation for the relief of aneurism, it has proved itself reliable, and may be cut short and left in the wound. As a suture gut is preferable to all others. It permits of a nice adaptation of the parts, is not liable to catch on the dressings when these are being removed, and can be left to take care of itself with no fear as to ultimate results. In the radical cure of hernia the pillars of the ring can be brought together with strong threads of catgut, and it is much superior to the very thick wire used by some surgeons for this purpose. Having operated recently on a case of this kind in Ward 29, I am in a position to say that it answered my expectations thoroughly, and the difficulty in the way of removal of thick wire constitutes one of the most serious drawbacks to its use; while the practice recom-

mended by Mitchell Banks of Liverpool, of leaving it buried in the tissues must occasionally give rise to after discomfort, for it is impossible that a heavy metallic suture, retained in such a position, can at all times accommodate itself completely to the parts in its neighbourhood. For the apposition of periosteum gut is indispensable, while hemorrhage from bone can be completely arrested by plugging the cavity with threads of this substance, and Lister's sinus forceps will be found the most efficient agents for accomplishing it. For all these purposes gut hardened in chromic acid should be used, and if prepared by the process recommended by Macewen it is thoroughly reliable, and will be found capable of resisting the action of the tissues for a considerable time. I have on more than one occasion found to my cost, that the material sold in shops, under the name of "chromic catgut," was in truth nothing of the sort, and gave way in the course of two or three days. (Dr. J. Whitson, Glasgow Med. Journal, Jan. p. 9.)

Kangaroo Tendon Ligatures.—For tying large vessels in their continuity the long even tendon from the tail of the kangaroo possesses decided advantages. It is as strong as the ordinary silk ligature, its knot is equally secure, and it causes no ulceration in the coats of the vessel. It has all the valuable qualities of the catgut ligature without any of the defects. A fine tendon makes also an excellent suture, it produces no more irritation than a gut suture, and it resists the softening effects of purulent discharges for a much longer period. A medium sized tendon (*e.g.*, about as thick as a catgut ligature which would be selected for the deligation of a large artery) is so strong that it is difficult to break it with your hands; both its strength and calibre are uniform throughout its entire length, which ranges from twelve to eighteen inches. It is owing to the peculiar flattened form, and perhaps also to the fibrous surface of the tendon from the kangaroo's tail, that a reef-knot tied in it retains its grip when left in the interior of a wound. Like catgut, tendons can be hardened, if desired, before they are employed as sutures in certain operations, by previous immersion, for about seven hours, in a half per cent. solution of chromic acid. This preparation may sometimes be necessary, but it was not adopted in any of the cases just referred to. When hardened by chromic acid they make excellent drains for wounds, and I have often so employed them. Their preparation for surgical purposes is most simple, but it is necessary to mention that they should not be removed from the tail *en masse*, they must be taken out one at a time, without force, or they will sometimes split longitudinally, which greatly deteriorates their strength. *A split tendon*

cannot be relied on, and should never be used. In their normal state they are of various sizes; some are thick, others very fine, in fact, every diameter which can be required is obtainable; there is, therefore, no occasion for splitting. Neither is it wise to twist two or more tendons together, as it destroys their flattened forms. (Mr. T. M. Girdlestone, Melbourne, Victoria, p. 269.)

Valvular Disease of the Heart.—*Digitalis.*—Digitalis is *facile princeps* of drugs in the treatment of imperfect compensation in mitral valve disease. The researches of modern observers—Fuller, Handfield Jones, Ringer, Balthazar Foster, Franks, Wood, Lauder Brunton, Milner Fothergill, and others—have shown its mode of action: that it so influences the cardiac ganglia as to induce a more perfect contraction of the ventricular muscle, and hence a more complete emptying of the ventricles; whilst at the same time, by an action of the vasomotor centre, it causes contraction of the arterioles and a heightened tension in the arterial system. It slows the heart by lengthening the diastolic pause. So not only does it give rest to the wearied cardiac muscle, but—as this muscle is nourished only during such diastolic pause by the blood which then enters through the coronary arteries—it directly ministers to its nutrition. It is a matter of common experience that digitalis, especially when combined with iron, strychnine, cod-liver oil, and other tonics, restores the *status quo ut ante* when in a patient manifesting a mitral systolic murmur the evidence indicates that compensation is beginning to fail. (Dr. A. E. Sansom.)

Belladonna.—Belladonna is only useful in the treatment of failure of compensation in cases of mitral regurgitation when combined with, or *occasionally* substituted for, digitalis. Belladonna, like digitalis, increases the power of systole, and raises the arterial tension. As Dr. Brunton has shown, it paralyses the cardiac terminals of the vagus, and reduces irritability by an anæsthetic effect on the sensory nerves of the heart. Very useful occasionally, it by no means compares with digitalis for prolonged employment. The hypodermic employment of digitaline 1-50th gr. with atropia 1-60 gr. I have found very satisfactory. (Dr. A. Ernest Sansom, p. 171.)

Convallaria majalis.—Dr. Sée considers that Convallaria majalis constitutes one of our most important cardiac remedies. It produces on the heart, blood-vessels, and respiratory organs, effects constant and constantly favourable; to wit, slowing of the beatings of the heart, with often a restoration of the normal rhythm; and, on the other hand, augmentation of the energy of the heart, and of the blood-pressure. Dr. E.

P. Hurd has published a remarkable case of Corrigan's disease, in which the fluid extract prepared by Parke, Davis, and Co., proved most beneficial, given in five drop doses every four hours. The therapeutical indications for the employment of convallaria are beginning to be generally recognised. It is useful in palpitation resulting from exhaustion of the pneumogastrics; in simple cardiac arythmia, with or without hypertrophy, and with or without valvular lesion; and in mitral constriction, with absence of compensation in the left auricle and right ventricle. It may be given with advantage in mitral insufficiency, especially when there is pulmonary congestion, with resulting dyspnœa. In Corrigan's disease, the peripheral arterial pulsations disappear, and respiration becomes markedly restored. In dilatation of the heart, with or without hypertrophy, convallaria is decidedly indicated. In cardiac dyspnœa, it is inferior to morphia or iodide of potassium; but, in some forms of asthma, it is said to be useful. Convallaria exerts no deleterious effects, and may be given with perfect safety. Dr. D'Ary says: "With me, it has long since taken the place of digitalis; and in cases of organic heart-disease, when, in the latter stages, the symptoms are becoming urgent, and the patient anxiously begs for relief, the physician will appreciate a remedy that will relieve signally and promptly, without the dread of overdose and cumulative action." At the present moment, convallaria is difficult to obtain; but in a few months there will be an ample supply. (Editor of British Medical Journal, p. 178.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

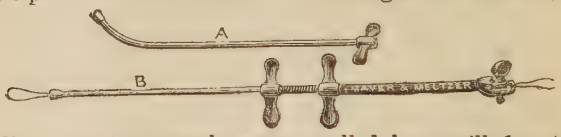
ASTHMA. — *Inhalation of Ethyl Bromide.*—Having had some experience of ethyl bromide since Dr. Squire brought it before the International Medical Congress last year, I have found it of great use in some cases of asthma supervening on old-standing bronchitis, and in one instance associated with mitral disease and anasarca. After many remedies, including inhalation of ether (which seemed rather to aggravate than to relieve), had been tried, the inhalation of ethyl bromide gave immediate and most marked relief. I have also used it as spray in some surgical cases for the removal of small tumours, and found that insensibility of the skin was produced, sufficient for the purposes of painless operation, without producing actual freezing of the skin. The preliminary red stage of freezing appeared to be sufficient, which was clearly an advantage. I believe inhalation of ethyl-bromide will be found of great service in obstetric practice, judging from my experience of it in other cases. (Mr. Spanton, Hanley British Med. Journal, Nov. 11, p. 934.)

Bronchial Asthma.—The turning point in the investigation of bronchial asthma was the discovery that irritation of the vagus causes the phenomena of asthma, not by acting peripherally—that is, through the branches to the bronchi—but by influencing the central extremity of the nerve, that is, the medulla, and so (reflexly) the muscles of respiration. When the *central* end of the divided vagus of the left side was faradised, and the other vagus cut, the same asthmatic phenomena were produced; the reflex, therefore, did not occur through the bronchial nerves, but by the respiratory nerves to the diaphragm and intercostals. That this was the case was completely proved by section of the phrenics before irritation of the vagus, for the phenomena of asthma were then entirely absent. An altogether unexpected result was thus reached, namely, that asthmatical phenomena may be produced *reflexly* through the vagus, and that the principal portion of the effect is a sudden inspiratory depression of the diaphragm, followed by its continued tonic contraction. It would thus appear that the theory of asthma, which represents the disorder as essentially one of bronchial spasm, must be given up. There can be no doubt that irritation of the vagus does cause bronchial spasm and moderate dilatation of the lungs, but this effect has always been regarded as much too insignificant to account for the symptoms of the disorder as clinically observed; and now that it appears to have been satisfactorily proved that besides this peripheral effect there is a reflex effect of incomparably greater importance, there is no reason why the theory of bronchial spasm should be any longer maintained. (Editor of Med. Times and Gazette, p. 205.)

Coryza.—*Sulphate of Atropia.*—According to Dr. Gentilhomme, sulphate of atropia (from a quarter of a milligramme to one milligramme, given as a pill) [say ℳss. to ℳij. of the *liquor*], has an immediate effect in the first stages of coryza, often arresting the progress of the disease. It also produces great relief when the coryza is confirmed, but its action is less remarkable than at the beginning of the inflammation. When bronchitis exists at the same time, the sulphate produces an equally favourable effect upon the bronchial mucous membrane. The employment of sulphate of atropia is based upon the fact that it has the power of lessening the nasal mucous secretion to the extent of complete arrest; and at the same time it acts beneficially upon the vessels by relieving their congestion. (Practitioner, Dec. p. 456.)

Nasal Polypi, etc.—*Jarvis's Wire Ecraseur modified by Dr. Jefferson Bettman.*—This modification of the Jarvis's snare,

shown at the International Medical Congress, was devised specially for the removal of nasal polypi and hypertrophic tissue covering the turbinated bones. Its action is simple and efficient, and, when properly used, it should occasion but little pain and loss of blood. In the original instrument, the



sliding or outer cannula was propelled by a milled nut or wheel, which has now been replaced by a flattened bar; this, for mechanical reasons, entails less expenditure of force, and can be manipulated with greater ease and comfort. One of the chief points in the modified écraseur consists in the clamp-screw to fasten the free ends of the wire loop. In Jarvis's instrument these were wound around small retention-pins, and, if the case required, had to be undone and rewound. This is obviated in the modification; a simple turn of the screw releasing or clamping the wire. Another point hereby gained is the impossibility of a fracture; the wire, except at its looped extremity, remaining straight throughout its entire length. The straight tube, which is used for operations in the nasal cavity, can be unscrewed from the handle, and replaced by a long tube with the postnasal curve, so that tumours or redundant tissue, growing from the posterior nares or in the naso-pharyngeal space, can be operated upon through the mouth. The wire used is the same as has been already recommended by Voltolini, *i.e.*, annealed steel piano-forte strings, Nos. 5 or 6. The instrument is made by Messrs. Mayer and Meltzer. (London Med. Record, Nov. 15, p. 480.)

Phthisis.—*Detection of Bacilli in the Breath.*—By the following simple method, I have succeeded in demonstrating with facility the presence of the bacilli of tubercle in the breath of patients suffering from true tubercular consumption; for which purpose, I allow the patient to breathe, at frequent intervals during the day, through two thin sheets of pyroxyline, or fine cotton, one layer in front of the other, and both of which are placed in the outer compartment of an ordinary 'pepper duster' respirator. The layer of cotton, when so arranged, will act as a double filter, the external layer removing from the ingoing air all suspended particles, such as dust, micro-fungi, polur, starch, &c., which are always more or less present in it, and which it is desirable to exclude; that portion of cotton which has been next to the mouth at the same time retaining those only existing in the outgoing

current, and which have been emitted from the lungs, viz., micrococci, bacilli, and some epithelial cells. It is in the latter layer only that I look for the organisms peculiar to this disease. This I do by converting the pyroxyline into gun-collodion by means of a mixture of ether and spirit. Every vestige of cotton-fibre is dissolved in the above menstruum, but other organic particles remain suspended in it. To render the bacilli manifest, my plan is to pour the thin collodion thus formed on a microscope-slide, and allow the fluid to run uniformly over the surface of the glass, then immediately placing the latter on one of its edges, that only the merest film of collodion may remain on the glass; the thinner the film produced, the more successful will be the experiment. The film is to be stained. This may be done by one of the methods well known to the profession for staining tuberculous sputum, such as that of Ehrlich or Heneage Gibbes. I have had excellent results from the former. Ehrlich's method is described in *Retrospect*, vol. 86, p. xliii, and Gibbes's method at p. xlv. (Dr. R. C. Smith, British Med. Journal, Jan. 20.)

Salicin.—The conclusions on the *rationale* of the action of salicin are certainly suggestive of its being made applicable as a curative means to that family of diseases whose intimate nature and dependence on pathogenic germs the experimental method has placed beyond the reach of cavil or doubt. It is not in salicin, in its simple state, that such a result can be looked for, but in those changes which it most assuredly undergoes while circulating in the blood and lymph streams, where, subjected to the influences of the animal chemistry, it is transformed into agents whose destructive energy on microphytic life none can question. That the resultant of those changes is an acid body endowed with such qualities, abundant proof is not far to seek, though this is not the place to adduce it. Till very recently it was generally believed that this acid was salicylic—a view which is no longer tenable if it be proved, as very recently affirmed by Dr. Stewart of Edinburgh, and resting on experiments made by himself, that the acid is, in reality, the carbolic. Let this be as it may, be it carbolic or salicylic acid, both agents really belong to a like category, and are both equally available in the treatment of disease of microphytic origin—the former by the method suggested in my letter to *The Times*, the latter by the internal use of salicin, both agents leading to a like result—the destruction of pathogenic germs. But it is in salicin alone, and not in its compounds, that we must look for the realisation of this object—in its compounds, no doubt, but only such as are fashioned in nature's laboratory; and not in the chemist's. (Surg.-Gen. Dr. W. Johnston, p. 203.)

Verbascum Thapsus, or Mullein Plant.—Dr. Quinlan speaks very favourably of the use of decoction, infusion, and extract of mullein in phthisis. Its extensive use among the peasantry of Ireland at the present day is but a relic of its former popularity, spread over a far wider geographical range. The evidence of old and modern authorities tends to establish its use as a very comforting demulcent, which may supersede ordinary cough mixtures, a great boon to phthisical sufferers with delicate stomachs. Dr. Quinlan says its weight-increasing (and, as it appears to me, distinctly curative) effect in pretubercular or early phthisical cases, has been hitherto overlooked. In *advanced cases of phthisis* mullein is merely a palliative; but it is a very powerful one. The appetite of the advanced phthisical sufferer is often so delicate, as to render it difficult to support life; and any little remnant of appetite is too often destroyed by the drugs which we have to administer for the relief of cough, dyspnœa, diarrhœa, or night-sweats. We can control the three former by the milk decoction of mullein, and the latter by hypodermic injection of atropia sulphate; and thus, by treating the case dietetically, leave the stomach free for nourishment only. I say dietetically, for the mullein decoction is so grateful and soothing that it comes to be regarded more as food than as physic. Some advanced cases find so much milk decoction heavy; but this difficulty is completely overcome by peptonising the milk with Benger's pancreatic fluid, and adding a pinch of sodium bicarbonate. The mullein decoction made with peptonised milk digests at once, and the slight bitterness of the peptone is completely covered by the flavour of the mullein. This is a decided improvement; and, in advanced cases, a necessity. The fresh leaves are the best, and can be procured nearly all the year. Excellent results, however, can be obtained with the dried leaves. The seeds can be had from Dr. John Evans, chemist, 49, Dawson Street, Dublin, who would also supply the leaves and their preparations. The seed should be sown very much like cabbage seed; as soon as the little plants spring up, they should be transplanted at proper intervals. The mullein is a hardy biennial, and will grow almost anywhere. (Dr. F. J. B. Quinlan, British Med. Journal, March 3, p. 435.)

Spasm of the Glottis.—*Nitrite of Amyl.*—I was sent for in haste to see an infant of nine months, which was thought to be suffocating. I found that the child had slept naturally, but about 6-30 a.m. had had a very slight spasm of the arms and legs, and at once began to breathe heavily, and became unconscious in about ten minutes: there was no cough at any

time. At 7 a.m. the hands and wrists, feet and ankles, were cold and purplish; lips livid; breathing very laboured, sixteen per minute; expiration much prolonged; sinking of whole chest-wall at each inspiration; pulse thready, and child quite unconscious. The use of cold having no effect in reviving the child, I sent one messenger for nitrite of amyl and another for assistance, with a view to possible tracheotomy. I caused the child to inhale the contents of two 5-minim pearls of the nitrite so gradually as to cause only slight flushing of face; almost immediately the stridor ceased, the breathing improved, the surface became warmer, pulse more distinct, and in perhaps two minutes the child was in a deep sleep, which lasted about twenty minutes. A hot bath, linseed poultice to abdomen, and internally potass. bromid. 2½ grs. every two hours, completed the treatment. Child was as well as usual next day. I report this case simply to bear testimony to the value of nitrite of amyl in an urgent case, and also because I am not aware of its having been used in this affection. (Dr. Joseph Williams, Boston, U.S., Practitioner, Dec. p. 446.)

Pleuritic Effusion.—*Tapping the Chest.*—The method of tapping the chest which I recommend, is by means of a medium-sized trocar (canula four millimetres in diameter), with tubing attached to act as a syphon. The trocar should work through an air-tight collar, and the canula have a branch to which the tubing is fitted. Into the syphon tubing a glass T-tube may be inserted for the purpose of attaching a side tube to be connected with a mercurial manometer, by means of which the exact intra-thoracic pressure may be observed at the commencement, the end, and at any time during the operation. The syphon tube should be long enough to provide a fall of one, two, or three feet, as may be required; and it is well to have the basin end of the tube fitted with a metal piece to which the nozzle of an aspirator may, if desired, be at any time attached.—The instrument to be used must be absolutely clean, and the canula and tubing should be filled with carbolised water (one to forty) before the commencement of the operation. If aspiration be preferred, Dieulafoy's, Bowditch's, or Potain's aspirator may be used.—*Choice of Spot for Puncture.*—The physician is responsible in choosing the site for puncture, and must not share the responsibility with others. In choosing the spot he has to be sure (1) that it is out of reach of diaphragm and heart; (2) that there is no adherent lung there. The *sixth space in the mid-axillary line* is the best point for puncture, and this point should be selected if, when tested by percussion, palpa-

tion, and auscultation, it proves satisfactory. This spot is most convenient because (1) most accessible whilst the patient is reclining in an easy posture; (2) the parietes are here moderately thin, and the intercostal space sufficiently roomy; (3) the mamma in females is out of the way; (4) we are sufficiently high up to be free from danger of perforating the diaphragm; (5) this point has the advantage over the point most commonly chosen, viz., below the angle of the scapula, in there being less probability of the canula becoming blocked by the flocculi, which tend from the position of the patient to gravitate towards the back of the chest (most of the dry tappings I have observed have occurred with the posterior puncture); (6) this point has the advantage over one chosen more anteriorly in being more central with regard to the effusion.—Of course, in special cases of limited effusion the point for puncture must be selected accordingly, it being remembered, however, that a central rather than the lowest point of the effusion should be chosen for puncture. — *Local Anæsthesia and Incision.*—The best means of employing local anæsthesia is to cut a plane surface on a lump of ice of about one inch area, dip it in salt, and apply with firm pressure to the spot chosen: in thirty seconds the surface will be frozen, and having been rapidly sponged, a small incision should be made through the skin, and the trocar will be easily thrust in without pain. The advantage of making an incision through the skin is that otherwise the force of the thrust is broken by the resistance of the skin, and the pleura is sometimes carried before the instrument.—The point to be chosen for incision in cases of purulent effusion is a different one from that to be preferred in serous effusion. A moderately low opening is desirable, in the seventh or eighth intercostal space, and *in the posterior axillary line.* My reason for recommending a lower and more posterior point for puncture in these cases is, that in acute empyema we wish to empty the pleural cavity of pus, and we look to obliteration of the abscess-sac by the descent of the lung as it re-expands and by the return of the heart to its normal position; these processes converge towards the lower and postero-lateral position of the chest.—Although there is no necessity to give chloroform, yet it is more convenient to do so, and patients with empyema bear anæsthetics very well. In operating be sure to make a good free opening into which you can introduce your forefinger, and thoroughly evacuate the fluid by the free admission of air. — Supposing the antiseptic treatment not to be strictly followed, all instruments and tubes used should still be steeped in carbolic lotion. The lighter and simpler the dressings used,

the better. A large piece of absorbent cotton-wool should be first laid over the tube for the discharge to soak into, and a pad of oakum applied over that, the whole being kept in place by a few turns of elastic bandage round the chest. For the first two or three days the dressings will have to be changed twice daily; then, if the drainage be perfectly free, the discharge will rapidly diminish and remain sweet, and one dressing daily will be sufficient. The drainage-tube must be removed at least once every two or three days, and thoroughly cleansed or renewed. The temperature of the patient must be carefully observed; it is the absolute test of the efficiency of the treatment. If there be any decided rise of temperature we may be sure that some accumulation of pus is present. Unless the contents of the pleura be fœtid, antiseptic injections are, in the first instance (and throughout, if there be no fœtor), best avoided, since they tend to retard the expansion of the lung. In those cases in which they are necessary they should be used with care. The pleura should never be *syringed out*, but an irrigator should in all cases be used. A tin pot or glass vessel, provided with a hole near the bottom, to which tubing is fitted, the tubing being supplied with a nozzle or pipe for insertion into the wound, is the simplest and best irrigator. The vessel, having been filled with the lotion required, is simply raised a foot or two above the patient, when the fluid flows into the chest equably, and with a uniform and measurable pressure. As it escapes from the chest the fluid is caught by a tray or other suitable receiver. Amongst the best disinfecting lotions may be mentioned—iodine, one drachm of the tincture to the pint of warm water; carbolic acid, 2 per cent.; Condy andw ater; quinine solution, two or three grains to the ounce; boroglyceride has recently been used in watery solution, one to forty. In the case of very young children who are terrified at injections, immersion in a warm bath coloured with Condy's fluid is an excellent plan suggested by Dr. Barlow and Mr. Parker. (Dr. R. Douglas Powell, Physician to Brompton Consumption and Middlesex Hospitals, Med. Times and Gazette, Dec. 9, p. 686.)

Throat-Sprays.—When Dr. Dewar introduced the treatment of different diseases about the throat with sulphurous acid spray, I used it very extensively. It certainly had great power in arresting inflammation of the surface, and in healing all ulcerated spots. I found, however, that it was very irritating in all cases where there was bronchial inflammation or an asthmatic tendency. So many people complained of the irritation, and children also disliked it so much, that I have

long since ceased to use sulphurous acid. The most agreeable, soothing, and useful application I have ever made in the cases referred to, is carbolic acid spray, made in the strength of one drachm of Calvert's carbolic acid to ten ounces of water. This should be used by a well made spray instrument, which will throw in spray, not water. I have been using this with great satisfaction ever since Dr. Richardson invented his spray-instrument. The first person who put me on the use of it, in consumption, was Dr. Purdon of Belfast. He also drew the attention of the late Dr. Stokes of Dublin to its use in consumption, who reported favourably of it. Carbolic spray has a pleasant taste, and is very soothing for a cough. (Dr. J. C. L. Carson, Coleraine, British Med. Journal, March 3, p. 442.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Cleft Palate.—To Mr. Thomas Smith, of St. Bartholomew's and the Children's Hospitals, is mainly due the credit of showing that it is possible to operate in infancy under chloroform, not only without risk, but with very great success. In his paper in the Med.-Chir. Trans. (*Retrospect*, vol. 59, p. 221), Mr. Smith described a gag by which the mouth of a patient can be kept open efficiently, and which, in some form or other, is essential for the due performance of the operation, now performed by him as follows. The patient is placed on a table of convenient height facing the window, and if possible a northern light. The head is supported by an air-cushion, and is firmly held by an assistant standing behind, while the arms and legs are strapped down to prevent struggling. The operator stands on the right of the patient, and the chloroformist on his left. The patient being thoroughly narcotised, the gag is introduced and the mouth screwed open, the rings of the gag being held by the thumbs of the assistant supporting the patient's head. The edges of the cleft are then pared with a slender double-edged knife, which is thrust through the margin of one side of the soft palate held tense with forceps, and made to cut up and then down to the end of the uvula. The margin thus separated is caught with the forceps, and the section completed up to the angle of the cleft, if possible, at one sweep, or if not, by a re-application of the knife. The same process is repeated on the other side; and, in favourable cases, it is both possible and satisfactory to remove the parings of both sides of the palate in one piece. The closure of the soft palate is then proceeded with, fine silver-wire being used, with horsehair or silk for the uvula. Mr. Smith employs a sharply

curved tubular needle for the wire, which is carried on a wheel in the handle of the instrument, and can be projected when the point has traversed both sides of the palate. A twister is employed to twist the wire up, but the last two turns are more conveniently given at the conclusion of the operation with a pair of torsion-forceps. The horse-hair is softened in warm water, and is introduced with a small curved or rectangular needle set in a handle, being passed through both sides of the palate, and caught with a "catcher" or forceps. The horsehair is simply tied with three knots, so as not to slip. Should there be a fissure of the hard palate, the operator proceeds to detach the muco-periosteum by a modification of Langenbeck's urano-plastic method. Making a puncture near the alveolus, he introduces a palate-raspatory of small size, and brings the point out in the fissure. Then, meeting this with a strong aneurism-needle, he replaces the raspatory with the needle, withdrawing the former altogether, and working with the needle from the fissure towards the alveolus. In this way, aided, if necessary, by the leverage of strong-curved scissors, the muco-periosteum of the hard palate is sufficiently detached; the hemorrhage, if troublesome, being controlled from time to time by a small sponge pressed against the palate. With the curved scissors, the soft palate is then detached from the bone of the hard, one blade being passed beneath the muco-periosteum, and the other above the soft palate, so as to divide the tissues transversely close to the horizontal plate of the palate-bone. The muco-periosteal flaps of the hard palate are now closed by a sufficient number of fine wire sutures; and, lastly, an incision, as recommended by Dieffenbach, is made on each side of the soft palate, so as to thoroughly loosen it, and take all tension off the stitches, which are then finally adjusted with torsion-forceps. (Mr. Christopher Heath, p. 271.)

DIARRHŒA.—*Eucalyptus Rostrata.*—The preparations found most useful have been a concentrated decoction (strength 1 in 20), and a dilute (1 in 40) made by boiling the powdered gum in distilled water for ten minutes, and filtering while hot, and a syrup (strength 1 in 3). For a moderately severe attack in the adult I commence with half an ounce of the dilute decoction every two hours. If after four doses no improvement results, the same quantity should be given every hour for four times, and if still little effect is apparent, I order half an ounce of the strong decoction every two or three hours. In the vast majority of suitable cases an abatement of the attack now occurs, going on rapidly to a cure, when the same dose

should be ordered every five or six hours, only gradually discontinuing the remedy. In an acute case it is best to commence with the strong decoction at the first, half an ounce every two hours, and at times every hour, increasing the time as above. The syrup may be given to children in doses of five to twenty or more drops three or four times a day, but, mindful of the tendency of sugar or mucilage to run into fermentation, seldom prescribe it, preferring small doses of the dilute concoction (thirty to sixty drops) guarded with spirits of camphor or some simple carminative every few hours, pushing it if needful. In many of the worst cases occurring in children, when all other remedies have failed, this alone has effectually stopped the alvine flux. As tannic acid diminishes the solvent power of gastric juice, the gum should not be given too near food. Its taste is rarely objected to, and can be improved by the addition of spirit of chloroform. Lastly, its cheapness is a desideratum, more especially in dispensary and hospital practice. (Dr. T. J. Hudson, p. 208.)

Excision of Enlarged Tonsils.—With respect to the method of operating, the tonsillotome has almost superseded the bistoury and tenaculum, owing to the ease and rapidity with which it can be used, and the little suffering it causes the patient. The rare case of a tonsillar calculus alone remains in which the tonsillotome is inapplicable. The instrument known as Physick's is preferred by some, that of Fahnstock by others, the majority; both models being of American invention. Physick's pattern has the advantage of allowing more force to be applied in fixing it firmly on the tonsil, as it is grasped by a stout handle, whilst the convex cutting blade can also be pressed down with great power by the thumb. Fahnstock's, on the other hand, is an instrument to be employed with dexterity rather than force, as it is manipulated by the thumb and first two fingers only, each of which is accommodated with a separate ring. It is, however, a much surer instrument than Physick's, which often fails to excise the tonsil, even in the hands of the most practised operators. This is mainly due to the aptness of the convex-blade, which enters at one point only, to press the tonsil outwards, and glide over or merely lacerate its surface, even when sharpened to perfection. The tonsil is, in fact, often very grisly, and requires to be jammed very firmly against the blade in order to be cut directly through. But the lunated edge of Fahnstock's cutting ring is much less likely to swerve from the straight course, as it begins by being in contact with the whole surface to be incised, whilst the organ is steadied and drawn into the tonsillotome by the trans-

fixing prongs. As Fahnstock's instrument is therefore almost certain never to miss the tonsil, it is more under the command of the operator, so that a larger or smaller portion can be removed according to the requirements of the case; whereas in using Physick's, owing to the necessity of fixing it as deeply and firmly as possible over the tonsil, the amount to be taken must mainly be left to chance. For the practitioner who seldom has occasion to excise the tonsils, Fahnstock's is decidedly the model to be recommended. In all cases it is advisable to support the tonsil during the operation from the outside of the neck, either by the hands of an assistant or by grasping the throat in a fork formed by the left thumb and forefinger of the operator. (Dr. Gordon Holmes, p. 279.)

HERNIA.—*Radical Cure of.*—Of late years there has been a revival of one of the oldest methods of cure—that by removing the hernial sac. That surgical bogie, the peritoneum, has been exposed; and we now know that, if not poisoned, it is a very tractable and inoffensive membrane. Indeed, even in cases of death after operation for strangulated hernia, it is very rarely that peritonitis kills the patient. Moreover, the confidence inspired by Listerism has made an astounding change in our views of the surgery of serous membranes; and to Lister is due the credit of having been early in the field, even in the department of curing hernia. I use thorough antiseptic treatment, and make a point of having the pubes, and parts around the anus, most carefully shaved. In inguinal hernia, the incision should commence at least an inch above the upper margin of the external ring, so that plenty of room may be given thoroughly to clear the pillars for the stitching. The sac is next freed from the surrounding tissues, and this is often much more troublesome than might be imagined. One is almost always tempted to think that it has been reached long before it really has; so that frequently, after a considerable amount of stripping has been done, it is found that it is not the sac at all that is being cleared, and the process has to be done over again. It is this mauling of the loose cellular tissue of the scrotum that gives rise to nearly all the trouble that occurs in the way of suppuration. The sac ought to be fairly reached before any stripping of it is done. Another point is that, in the case of an old sac, the lowest point is intimately adherent to the tunica vaginalis; and, if it be roughly pulled upon, the testicle enclosed in the tunica comes bodily out of the scrotum—not a very serious matter, it is true, but unpleasant to look at. The sac having been cleared, its contents are pressed up into the abdomen.

When it is thin, there is no difficulty in making sure that it has been completely emptied; but, if there be the slightest doubt, it should be slit up and its interior examined. Adherent omentum, if in small quantity, I separate carefully, tie with catgut in one piece, and cut off; if in large mass, I split it up into two or three portions, and ligature with carbolised silk to ensure a good knot that will not slip. One cannot be too careful about the securing of the omental stump before it is finally pushed into the abdomen, and every drop of bleeding should have ceased, both from the omentum and from the neck of the sac, before the next proceeding. This consists in pulling the sac well down, and tying it as high up as possible. I use two ligatures of strong catgut, as that material is apt to be treacherous. In case anything should happen to one, the other is there. With a curved needle, armed with strong silver wire, I next pull together the pillars of the external ring, leaving only room for the spermatic cord at its lowest part. Two or three stitches suffice. These are cut short off, and left *in situ*. A clean carbolised sponge, put beneath antiseptic gauze for the first twenty-four hours, makes the best dressing. The only point of novelty for which I take any credit to myself in this operation (and very likely others have done it as well as myself) is the use of strong silver wire to draw together the pillars of the ring, which is left permanently in position. Catgut I distrust, where there is any strain upon it; it yields too soon. Silver-wire seems to bury itself so harmlessly, that I cannot see any objection to it; while it must of necessity hold the pillars together for a considerable time, until they are well agglutinated. I tried magnesium wire once, with the idea of its oxidising and disappearing, but it was too brittle; and, after all, silver wire is just as innocuous. In the case of femoral hernia, I content myself with mere removal of the sac, as the introduction of sutures to pull down Poupart's ligament to Gimbernat's would be very troublesome, while there is not the same necessity for suture that there is in inguinal hernia. The femoral rupture is very rarely so large as the inguinal, and, consequently, the femoral ring is not dilated so enormously as the inguinal canal is, while its walls are more rigid and unyielding. (Mr. W. Mitchell Banks, p. 285.)

The patient having been put under chloroform, and the pubes carefully shaved, I proceeded to operate by first of all transfixing a fold of skin over the inguinal ring, and cutting outwards. The various structures between the skin and the mouth of the canal were successively divided on a director until the sac was reached, when it was opened into, and a

large portion of it removed. Wood's needle, threaded with the strongest chromicised catgut, was then passed through the upper and outer side of the ring, carried over towards the inner pillar, and when brought into contact with the skin on that side, the latter was pulled away in the direction of the mesian line, so as to permit of the exit of the point of the needle at the opening already made. Three separate sutures of catgut were introduced in this way; and the cut edges of the sac were stitched together with moderately sized gut. The pillars of the ring were then firmly approximated by tightening the sutures, and securing them with reef-knots. A decalcified drainage tube, threaded with horse-hair, was inserted into the wound, the margins of which were brought together with two button sutures, and accurately apposed by means of several stitches of fine gut. Protective plaster and gauze were next applied in the usual way, and an elastic bandage assisted in keeping the whole in excellent position. The dressings were changed on the second day, when the hair was withdrawn from the drainage-tube, and union by first intention had taken place along the whole line of incision, with the exception of the lowest point, where a necessary aperture for the exit of the discharge remained. The wound followed a truly aseptic course, and the patient made an excellent recovery.—Catgut, if prepared by the process recommended by Macewen, and kept in solution for a week, will be found thoroughly reliable. Catgut has many advantages over wire. It possesses greater pliancy, and adapts itself much more readily to the position of the parts with which it comes in contact: a firmer and better knot can be tied with it than by means of a metallic suture, and one which takes a closer and more equable grasp of the structures which it surrounds; and on account of its bland and innocuous qualities it never causes irritation, while at the same time, from its ultimate absorption, it cannot possibly produce after-discomfort.—*Macewen's method of preparing chromicised catgut* is as follows: One part of chromic acid is first added to five of water. One part of the solution thus made is then mixed with five of glycerine, and if the gut is steeped in this for a week, it will be found capable of resisting the action of the tissues for a fortnight. (Dr. James Whitson, p. 288.)

Position as an aid to the Reduction of Irreducible Herniæ.—I had a mattress placed on the floor, and got my patient to slip off the side of the bed so that she rested on her elbows on the mattress, her sister and nurse, standing on the bed, holding her up by her legs. The first day she could not bear this position for more than two or three minutes twice in the day;

the second day she three times remained for five minutes. On the third day I saw her, and finding no change I recommended that she should support herself sideways on her right shoulder and arm, one attendant supporting her round the waist, another taking the legs, and a third keeping the left side higher than the right, at the same time that with one hand she gently pushed the tumour in an upward direction towards the feet. For the next two days this was tried three times for five minutes at a time. On the third day, on the third trial (which was continued for seven or eight minutes) it yielded, being, as she described it, slowly dragged into its place. A truss was then fitted, and in a few days she was moving about as usual. (Mr. Buxton Shillitoe, Lancet, Dec. 23, p. 1073.)

Strangulated Hernia.—I lay the patient on his back, and make him draw up his knees. In inguinal hernia I support the scrotum on a pad or pillow, and pour one or two tablespoonfuls of ether over the integuments of the sac; I then lay over it a cloth folded three or four times. This I repeat every fifteen minutes. After the third or fourth repetition, the coils of intestine become mobile, and sometimes slip back of themselves into the abdomen. If they do not do so I attempt to reduce them gently; and in fifty-four cases out of fifty-eight I have succeeded. I find the treatment more successful the less time is previously spent in taxis: so I apply the ether as soon as possible after the strangulation. It is easy to see why. When the bowel has been nipped for some time, it becomes paralysed and loses its power to contract and to conduct peristaltic movements. Thus your chance becomes less hopeful or is lost. I had no instance of simply omental hernia in these fifty-eight cases. I should think in such a case etherisation would be useless. In cases where the hernial coverings were much thickened it would probably be less effective. To lessen the burning pain produced by the ether, a mixture of 100 parts of sulphuric ether with 20 of oleum hyoscyami is used. The ether must be poured on, not dropped. (Dr. W. Finkelstein, Jassy, Practitioner, Nov. p. 365.)

In the reduction of an incarcerated hernia, the best of all accessory means is the production of deep anæsthesia by chloroform. Baths, tobacco clysters, ice-bags, &c., only serve to waste time in such cases. If reduction fail under deep narcosis, operative meaaures should be undertaken there and then. Taxis should never be used to excess. The dangers of operation are less serious under antiseptic precautions than are those of forced taxis. Small painful and inflamed incarcerated herniæ should be operated on at once.

With the larger painless and non-inflamed tumours, there is always less urgency. (Prof. Nussbaum, Medical Record, Nov. 15, p. 443.)

Fissure of the Anus.—*Iodoform.*—Dr. Boardman Reed, in the *Philadelphia Med. News*, observes that he has found iodoform very efficacious in this troublesome affection. The formula he employs is: Iodof. pulv. 3 ss., balsam Peruv. 3 ij., cosmolini 3 j. To be applied three or four times a day after washing the parts. The balsam of Peru much disguises the offensive odour of the iodoform, and probably adds to the healing power of the ointment. In treating anal fissures by this or any other means, laxatives sufficient to produce one soft stool a day should be employed; but anything approaching purgation will be likely to aggravate the congestion, and prevent a cure. (Med. Times and Gazette, April 7, p. 377.)

Fistula-in-Ano.—*Operation for.*—On the day previous the bowel should be cleared by a dose of, say, 6 drachms of castor-oil with two drachms of tincture of rhubarb, and on the morning of the operation an enema of plain warm water should be administered. Cutting a fistula seldom demands the use of anæsthetics, unless in very nervous individuals, some of whom insist on having them. The external opening should be sought for. This may be close to the verge between two of the rugæ; it may be concealed by a sentinel papilla, such as we see leading to cloacæ over diseased bone; or it may be any distance out to the tuber ischii. Velpeau's probe director being passed through the fistula until it enters the bowel, the forefinger, well oiled, should enter the rectum; by gentle manipulation the point should be hooked down until it projects through the anus. The instrument having a flat handle can be securely maintained with the groove in the proper position. A strong sharp-pointed knife should be rapidly passed along this, dividing the tissues bridged upon it. Should any difficulty exist in finding the internal opening, the search may be assisted by the finger in the rectum. The proposal to inject ink or milk for this purpose can seldom be necessary to anyone who has any use of his hands, and it only complicates and prolongs the operation. Now, you may ask me the question—Does it make any difference whether we introduce the finger or the probe first? I believe it does make a great deal of difference, both to the surgeon and to the patient. The finger, if introduced first, must press the walls of the fistula together, and by thus closing it obstruct the passage of the probe along its track. The operation of Pott consists in the introduction of the finger, then the passage of a probe-pointed bistoury along the fistula,

hooking it out through the anus, and cutting the tissues lying over the edge of the knife. This method is open to three objections—the edge of the knife may touch the fistula and cause the patient to start, the knife (if of ordinary construction) may break, the surgeon may cut his finger. Should the inner opening be too far from the verge of the anus to admit of hooking down the probe, a lithotomy gouget may be slipped over the finger, so as to protect the opposite wall of the gut, and the division may then be effected along the groove of the director. If assistants are not to be had—as may often happen in country districts—a large tallow candle may be passed into the bowel, and the knife, having been pushed along the fistula, may be plunged into it, dividing the necessary tissues, without any risk of wounding the opposite wall. In cases of "*complex*" fistula, or in those where the track runs very far from the bowel, it is not necessary to divide all the parts intervening between it and the mucous membrane. Such a proceeding would sometimes involve a dangerous gash in its wall, and might be followed by a very troublesome result—incontinence of fæces. Should there appear to be no internal opening, and the probe is felt through the thin mucous membrane, it is good practice to push it through, making an internal opening, and then completing the operation. In cases of complex fistula it is not advisable to lay all the channels open by extensive incision. A free outlet may be made for treatment by injection and drainage. The operation for fistula may then be completed. After operation the wound should be lightly dressed. A small piece of lint, well oiled, should be passed first into the bowel, and from that into the wound, so that it shall lie in it from top to bottom, through its entire extent. Now, it makes all the difference whether you pass the lint into the wound or into the bowel. In the former case you are likely to pass the pledget obliquely through the lower part of the cut into the rectum, leaving the upper part to heal by direct union, and thus lay the foundation for a return of the disease. The application of a bladder of ice or a cold sponge gives great relief from the smarting pain and diminishes the likelihood of hemorrhage. An instrument has been devised for irrigating the rectum with cold water, but the ice bag is more effectual. (Mr. E. Hamilton, p. 290.)

Prolapsus Ani.—*Hazeline.*—A widow lady, aged about forty, has suffered for several years with prolapsus ani of a severe character, attended with frequent hemorrhage. About a fortnight ago the bleeding was very profuse, so much so that wine and brandy had to be given to her frequently to keep

life in her. All remedies had been tried by her medical attendant, and failed to arrest the hemorrhage for any time. I wrote and advised them to try hazeline, and found it had just been tried with great success, all bleeding having stopped, and there has been no return of it. The patient is recovering. (Mr. G. Hother, Lewes, Lancet, Dec. 20, p. 971.)

Hypodermic Injection of Ergotine.—Dr. Jette describes what he has observed of this treatment, first introduced at the St. Louis Hospital by M. Vidal. The formula is composed of one gramme of Bonjean's ergotine and five grammes of cherry-laurel water. It should be freshly prepared, so as to be of a limpid colour and fresh and agreeable odour. The injections are made every other day, commencing with fifteen drops, and, if this is insufficient, going on to twenty or twenty-five. This is inserted very slowly into the fibres of the sphincter to the depth of from two to four centimetres. Severe lancinating pain is caused, which then becomes dull and continuous, only ceasing after some hours. After three or four injections contractions of the sphincter and fibres of the intestine commence. A sense of constriction and traction upwards is felt by the patient and observed by the surgeon. Various disturbances may take place; thus, if the doses of ergotine are too weak, there may be frequent desires to go to stool and to pass urine; and when they are too strong there may be spasm of the neck of the bladder, with temporary retention of urine. In some persons M. Vidal observed a tendency to vertigo or syncope, and painful præcordial constriction, with a hard, close, somewhat slow pulse. The latter injections become more active, as if cumulative, and cause a more prolonged tenesmus than the early ones. The duration of treatment oscillates between some days and some weeks, and seems quite independent of the solution employed, the dose injected, and the intervals observed. To render the cure certain when once obtained, it is of importance that three or four supplementary injections should be made. (Med. Times and Gazette, April 7, p. 377.)

Tapeworm.—*Oleoresin of Male Fern: Increasing its Efficacy against Tapeworm.*—According to E. Dieterich, the frequent failure of oleoresin of male fern as a remedy against tapeworm is to be ascribed to its irrational administration. It has become known that the popular 'worm-doctors,' who use almost exclusively the oleoresin of male fern, and who hardly ever meet with a failure, administer the remedy in conjunction with castor-oil, instead of following it by the oil after one or two hours, as is usually done by practitioners.

The object is to bring the extract, in an unaltered or undigested condition, into contact with the worm. The experiments which have been made by mixing one part of the oleoresin with two parts of castor-oil have been very successful, and this mode of administration deserves, therefore, the preference. Oleoresin of male fern is apt to derange the stomach, and, when enveloped partly in the oil, is likely to pass it more rapidly, which constitutes another advantage. The mixture has, it is true, an unpleasant taste. This may, however, be disguised by filling it in capsules of about 45 grains each. The dose may be regulated from six capsules to seven or eight more, according to circumstances. It is advisable to empty the bowels the preceding day by a mild purgative, best by castor-oil. (London Med. Record, Nov. 15, p. 482.)

TONGUE.—*Gouty Psoriasis and other Inflammatory Conditions of.—Chromic Acid.*—Sir James Paget says that a most useful local treatment of gouty psoriasis of the tongue is to paint the white patches with a saturated or very strong solution of chromic acid. I have employed chromic acid in the treatment of several kinds of inflammatory conditions of the tongue, and in certain cases with most marked success. The following conclusions may be drawn: Chromic acid cures with marvellous rapidity *secondary* affections, ulcers, mucous tubercles, condylomata. It produces no appreciable effect on *tertiary* affections, gummata, extensive ulcers, tubercular syphilides. Some cases of chronic superficial glossitis, where slight ulceration and renewed inflammation has occurred, improve quickly under its influence. Other cases of glossitis, in which the tongue surface is attacked by a fresh inflammation of great severity, so far from improving, appear actually to be rendered worse by chromic acid. Glycerine of boracic acid and soothing remedies are more suitable for such conditions. The strength in which the chromic acid has been almost invariably employed has been ten grains to one ounce of water. In a few instances fifteen grains have been ordered. The patient has been told to paint the diseased portions of the tongue three or four times a day with a camel's-hair brush dipped in the solution. Pain or discomfort from the application has seldom been complained of; and even if there has been a little smarting at first, this has cheerfully been borne on account of the relief which the lotion has afforded. I hope at some future time to be able to describe the effect of chromic acid on syphilitic affections of parts other than the tongue and lips. At present I can only say that it is very useful in secondary affections of the tonsils and the palate. (Mr. H. T. Butlin, p. 276.)

AFFECTIONS OF THE URINARY SYSTEM.

ALBUMINURIA.—*Chloral.*—Like all new drugs, chloral has been used in various combinations for the most varied affections. No one, I think, has as yet drawn the attention of the profession to the almost marvellous effect of chloral in causing albumen to disappear from the urine, and with it the presence of an existing œdema. From the regularity with which it was noticed that the urine passed after taking chloral was clearer, of lower specific gravity, and contained less albumen than that passed at other times, it was decided to test the effect of chloral by withholding its administration altogether. Until now, the patient had been improving; the albumen had greatly diminished, the œdema was disappearing, and the patient was able to be moved to the couch; but no sooner was the chloral stopped than the symptoms returned. Every medicine was now stopped, with the exception of the chloral, as it was quite apparent to Dr. Oliver and myself that this was the only remedy likely to prove of service. Daily I made a comparative examination of the urine passed at various periods, and I always noticed that the urine which was passed after taking chloral contained a diminishing quantity of albumen. Dr. Oliver at this time made an independent examination of the urine, of which the following is a brief statement: Specimens passed after taking chloral were of average specific gravity 1,016, acid, with no albumen; specimens passed at other periods contained albumen, and granular and hyaline tube-casts. A continuation of the chloral treatment resulted in complete disappearance of albumen from the urine, and with it disappearance of the other symptoms I have mentioned. In the middle of the month of July she had so far recovered that she was able to be removed to the sea-side. At the present time she is better than she has been for many months past, and, with the exception of amenorrhœa, she is quite well. No explanation is offered as to how the chloral was followed by such beneficial results. Suffice it to say that, under its use, a lady so prostrate that she could not stand, with a dilated heart, albuminuria, and marked œdema of feet and legs—indications of a grave constitutional state—has simply been rescued from death. The chloral did not produce any apparent diuresis or diaphoresis. (Mr. Thomas Wilson, p. 226.)

BEDSIDE URINARY TESTS.—From the numerous contributions on portable urinary tests which have recently appeared, it is clear that practical men, who have long realised the serious inconvenience of carrying about caustic fluids for testing at

the bedside, are anxiously feeling their way to more manageable and handy yet equally trustworthy reagents. My experiments have embraced the qualitative and quantitative testing of albumen, sugar, and total acidity. I should make the preliminary statement that I have succeeded in all my reagents in abolishing the fluid state, and likewise the solid form, either of powder, crystal, or pellet. It occurred to me some time ago that evaporation of the test fluids I was then using on chemically inert filtering-paper, linen, or other similar fabric, would secure the deposition of the reagents in a finely divided and concentrated state, a condition it was hoped favourable to such a rapid re-solution of them in the urine as to produce a quick and sensitive action on the constituents sought for. I soon discovered that my pieces of chemically charged paper were, when dropped into a small quantity of the urine in a test tube, very delicate and cleanly tests; and being in the most portable and compact of all forms for clinical work, and, moreover, affording better results than I had previously obtained from the old corrosive test solutions, it was not long before I cleared my spoilt urinary case of the latter; and I can assure my readers I did so with a feeling of satisfaction and comfort. Then, inasmuch as it was an easy matter to graduate the papers with standard solutions of the reagents, I next proceeded to inquire how far this simple process could be carried in determining the quantities of albumen, sugar, and total acidity; in other words, I thus attempted to apply at the bedside the volumetric method of analysis in the form of pieces of filtering paper charged with definite quantities of the reagents with a quantitative colour limitation on paper from which the percentage of the constituent sought for could be at once read off. Up to the present I am satisfied in having attained rapid and, for all practical purposes, sufficiently accurate results. (Dr. G. Oliver, Lancet, Jan. 27, p. 139.)

Albumen Precipitant Test Papers.—Dr. Oliver saturates filtering paper with potassium ferrocyanide, potassio-mercuric iodide, picric acid, and other reagents, and then proceeds as follows: *Citric Acid.*—All the foregoing reagents are inoperative as albumen precipitants unless the urine is highly acidified; their application should therefore be preceded or accompanied by a sufficient charge of acid. For this purpose citric acid is easily made available when deposited to saturation on filtering paper, and in this form it has afforded me uniformly satisfactory results with all the albumen test papers. *Compound papers.*—Instead of using citric paper separately prior to he reagent paper, it has been combined by a thin

layer of rubber with the latter as a single test paper in the case of sodium tungstate and of potassio-mercuric iodide. *Picric acid* can be deposited to saturation on filtering paper, which becomes a most compact and cleanly vehicle, and which, moreover, quickly delivers its charge to water. Repeated observation has shown me that when united with citric acid, as in the test papers, picric acid is divested of all the objections that have been urged against it. A few drops of albuminous urine instantly turns the bright picric solution, extemporaneously prepared from the test paper, into a muddy one, while the addition of more urine does not redissolve the precipitate as when picric acid alone is used. For the detection of small quantities of albumen (less than 1 per cent.) the "contact" method of testing is necessary; then it is best to make the picric solution in a wineglass, to take it up by the medicine dropper, and to glide it gently over the urine in the test tube. *How to use the test papers.*—About thirty minims of the urine are taken up by a nipple pipette, or medicine dropper, and transferred to a short test tube, preferably one about two inches in length. If turbid from urates, it should be gently heated. It is now strongly acidified by dropping into it a citric paper, which is shaken about for a few seconds, and may then be withdrawn or allowed to remain. It is not now necessary to ascertain if the urine is sufficiently acid, therefore without delay the test paper selected is allowed to fall into it. A simpler plan, and one which I find answers equally well, is to drop both the citric and the reagent papers into the urine, so that they may fall together to the bottom, and to one side of the test tube. The latter is now inclined, so that the urine may repeatedly and slowly flow over the paper; when, if albumen be present in small or medium quantity, a whitish cloud will very quickly gather above and below it, the more readily detected by intercepting the light by the hand, &c.; while, in striking contrast, the upper part of the urine will remain clear. If, however, the albumen exists in large proportion, it will not usually produce a haze about the paper, but will coagulate on it, and will slowly fall from it in clots. Then, in any case, on shaking the tube the urine will become less or more opaque, according to the amount of albumen present. If, on the other hand, the urine preserves its brightness, or if any turbidity it possessed prior to the introduction of the test paper is not increased, it may be inferred it is free from albumen. But inasmuch as it is just possible, though I have never found it so, it may not have been sufficiently acidified to enable the reagent to throw down the albumen, it is advisable to dispose of this suspicion by adding another citric paper; when, if no precipitation

occurs, albumen is absent. The whole proceeding, of course, takes up very much less time than that occupied in reading this description of it. The reaction is practically instantaneous when the urine has been freely acidified prior to the introduction of the test paper. It is, however, not so quickly obtained, though the delay only amounts to a few seconds, when, without previous acidification, the single compound test papers are used. The convenience and simplicity of the testing for albumen by these combined papers are very great, for the practitioner only requires to carry them in his visiting list or pocket book and to drop one into a little urine in a teaspoon or wineglass, when, after stirring it about for a second or two, the opacity of precipitated albumen will appear. Those who prefer to develop a zone of precipitation along the plane of contact of a test solution and the urine, can do so by aid of these papers. A test tube and a wineglass are required. Into the latter the reagent paper rolled up is placed with about fifteen minims of water, and, without shaking, is set aside, while a similar quantity of urine is put into the test tube with citric paper. After withdrawing the latter, the reagent, now in solution, is taken up by the pipette and is allowed to trickle down the side of the tube, in which it will collect at the bottom. After developing the ring, the two fluids may be shaken together, when the albumen will be more largely precipitated as a milky cloud. (Dr. George Oliver, Harrogate, Lancet, Feb. 3, p. 191.)

Ferrocyanic Test Pellets as a Clinical Test for Albumen.—Yellow prussiate of potash and acetic acid, employed together, have long been known to furnish a valuable test for albumen. There may be other tests as good, but I think it may be said that there are none that can be spoken of as actually better. Citric acid may be made to take the place of acetic acid, and thus a test capable of being kept and used in a solid form is supplied. The precipitant of the albumen is ferrocyanic acid, and this is liberated just as effectually by citric as by acetic acid. I, at first, thought that it would suffice simply to mix the yellow prussiate of potash, and citric acid, in the proper proportions, and compress into a pellet to obtain the test in the form I wanted. Experience, however, soon showed me that the matter was not so easily to be disposed of. Difficulties presented themselves which have taken time and consideration to overcome. With the willing and able assistance, however, which Mr. Cooper has rendered in carrying out the mechanical operations I have suggested, a pellet has been produced which seems, as far as I can at present judge, to supply all that can be desired. Its components are the sodic ferrocyanide, and citric acid. Grounds exist for the employment of

the sodic instead of the potassic ferrocyanide. All that is necessary in using the pellet is to crush it to a powdered state, within a folded piece of paper, with a silver or other coin from the pocket, or in any other way that may suggest itself, and to run the powder into an ordinary sized test-tube, and pour in the urine to be examined to the height of about an inch. On simply agitating freely, without the application of heat, a precipitate will immediately, or almost immediately, appear when albumen is present. The test is so delicate, that the smallest amount of albumen gives rise to a distinctly recognisable opalescence, and with a larger quantity, a dense white precipitate is produced. Instead of crushing the pellet, it may be broken in half, or placed in a whole state in the urine. Used in this way, it takes a minute or so for it to be dissolved, and the reaction to be produced. An estimate may be formed of the amount of albumen present by allowing the precipitate to settle, and reading off its height in proportion to the contents of the tube, in the same way as is done after the application of heat. As no employment of heat is required in the application of the test, it is not necessary that a test-tube should be used. A wine-glass or medicine bottle will answer instead, and the quantity of urine should be kept down to about that recommended when a test-tube is used. Enough acid exists in the pellet not only for liberating the ferrocyanic acid from the ferrocyanide, but for more than neutralising the alkalinity that is likely to belong to a specimen of urine limited to the quantity which has been recommended to be taken. Through this circumstance, the test acts equally well with alkaline as with acid specimens of urine. Since this communication was written I have seen the albumen precipitant test papers introduced by Dr. Oliver, of Harrogate. They certainly form a very neat and elegant adaptation. Whilst encountering the difficulties that presented themselves with the production in a satisfactory state of the ferrocyanic pellets, the idea crossed my mind of papers soaked separately in the two agents and dried, but I did not act upon it, as I thought the presence of the paper in the test-tube would be undesirable, and that it would be best, if possible, to keep from any extraneous substance. In the pellets there is nothing besides the two agents actually constituting the test, and their nature is such as to be perfectly harmless in every way. Properly preserved in a bottle I have no reason, from the opportunity I have yet had of judging, to think otherwise than that they will keep for an indefinite time. They are made by Mr. Cooper, of 58, Oxford Street. (Dr. F. W. Pavy, British Med. Journal, Feb. 17, p. 308.)

Picric Acid as a Test for Albumen in the Urine.—The test may be used in the form of a saturated aqueous solution, or in the form of powder or crystals. The aqueous solution is most suitable for home use, while the powder or crystals may conveniently be carried in a urinary pocket test-case. A saturated aqueous solution may be quickly made by adding about fifty times their bulk of boiling distilled or rain-water to the powder or crystals; a portion of the acid will crystallise out on cooling, leaving a transparent yellow supernatant liquid. This solution, being added to an equal volume of albuminous urine in a test-tube, immediately coagulates the albumen. The coagulated picrate of albumen is soluble in alkalies; if, therefore, the urine be highly alkaline, it must be acidulated by a vegetable or a mineral acid before adding the picric acid solution. In my numerous testings for albumen with picric acid, I have not once found it necessary to acidulate the urine. The picric acid solution is itself sufficiently acid to dissolve the phosphatic sediment which results from boiling a neutral or alkaline specimen of urine. To detect a very minute quantity of albumen, the following method is the best:—Into a test-tube about six inches long the urine is poured to within two inches of the top; then, the tube being held in a slanting position, about an inch of the picric acid solution is gently poured on the surface of the urine, where, in consequence of its low specific gravity (1003), it only partly mixes with the upper layer of the urine; and, as far as the yellow colour of the picric solution extends, there will be more or less turbidity from coagulated albumen, contrasting with the pellucid unstained urine below. If, then, the tube be placed in a stand, the coagulated albumen will gradually subside, and form a delicate horizontal film at the junction of the coloured and the unstained stratum of urine; the yellow liquid above and the uncoloured urine below being quite free from turbidity. If the urine should be turbid with urates, it must be cleared by heat before the addition of the picric acid solution. As a result of numerous careful observations, I have arrived at the conclusion that picric acid applied in this way is a more delicate, and therefore more trustworthy, test for albumen than nitric acid in cold urine, whether the latter be employed by the method of dropping the acid into the cold urine, or by pouring the urine on the acid previously placed in the tube. The simplest and most satisfactory mode of comparing the two tests, as regards their relative delicacy, is to dilute a specimen of albuminous urine until one or the other test fails to act; and it will be found that the picric acid solution shows the presence of albumen in a specimen diluted considerably beyond the point at which the nitric

acid fails to give any indication. The picric acid, too, often causes an immediate albuminous opalescence in specimens in which nitric acid only slowly, and after an interval of some minutes, gives a similar, but sometimes a doubtful indication. It scarcely need be insisted on that, for example, during convalescence from acute albuminuria, it is of the greatest practical importance to be assured that no trace of albumen remains. (Dr. Geo. Johnson, Med. Times and Gazette, March 24, p. 322.)

Picric Acid as a Test for Sugar.—The value of picric acid, as a test for clinical use, is much increased by the fact that, when boiled with a solution of potash, it forms a most delicate test for glucose. Take a fluid drachm of a solution of grape-sugar, in the proportion of a grain to the fluid ounce; mix it with half a drachm of liquor potassæ (P.B.), and ten minims of a saturated solution of picric acid; and make up the mixture to four drachms with distilled water. The mixture is conveniently made in a boiling tube, ten inches long and three-fourths of an inch in diameter, which may be marked below at the height of two and four drachms. With a long boiling-tube there is little risk of the liquid boiling over; and the steam, condensing in the upper cool part of the tube, flows back as liquid, so that there is little loss by evaporation. The liquid is now raised to the boiling point, and the boiling is continued for sixty seconds by the watch, so as to insure the complete reaction between the sugar and the picric acid. During the process of boiling the pale yellow colour of the liquid is changed to a beautiful claret red. The liquid having been cooled by cautiously immersing the tube in cold water, and it having been ascertained that its level is that of the four-drachm mark on the tube, or, if below the mark, it having been brought up to it by the addition of distilled water, the colour is that which results from decomposition of picric acid, by a grain of sugar to the ounce, four times diluted; in other words, it indicates one-fourth of a grain of sugar to the ounce; and this colour is a convenient standard for comparison in making a volumetric analysis. The picramic acid solution, however, on exposure to light, even for a few hours, becomes paler; but the colour may be exactly imitated by a solution of ferric acetate, with a slight excess of acetic acid and an excess of ferric chloride. The iron solution we have found to retain its colour unchanged for a fortnight, even when exposed to a strong light; and we expect that, when light is excluded, it may be kept for an indefinite period; and it is, therefore, a convenient standard for comparison. If, now, a drachm

of a solution of grape-sugar, containing two grains to the ounce, be mixed with the same quantity of liquor potassæ (half a drachm) as before, but with double the amount of picric acid (*i.e.*, twenty minims), and made up to four drachms in the boiling-tube, the result of boiling the mixture, as before, for sixty seconds, will be the production of a much darker colour than when the one-grain solution was acted upon; but if now the dark liquid be diluted with its own volume of water, the colour will be the same as that of the one-grain solution. Dr. Johnson has had a '*picro-saccharimeter*' made for this test by Mr. E. Cetti, 36, Brooke Street, Holborn, E.C. (Dr. George Johnson, Med. Times and Gazette, March 24, p. 323.)

Metaphosphoric Acid as a Test for Albumen.—Metaphosphoric acid as one of the tests for albumen was recommended by Dr. Grigg in 1880. A piece about the size of a split pea dropped into the urine in a test-tube affords, he says, on the application of gentle heat, a ready and certain test. As the acid dissolves, the albumen forms a white cloud around it. (Mr. F. P. Atkinson, Lancet, Feb. 10.)

Convenient and Delicate Method of Testing for Albumen.—Dr. A. W. Abbott, of Minneapolis, U.S., gives the following description of an easy and delicate method of testing albumen. "Pour a few drops of urine gently down the inside of a glass vessel containing acidulated water at the boiling point. If albumen be present, a more or less dense cloud will form, just at the dividing line between the fluid tested and the clear water above. As the contrast in opacity is between the clear water and the milky albuminous cloud, the test is very delicate, one-twentieth of one per cent. of albumen making a very perceptible clouding. It has all the advantages of the ordinary heat and acid test and Heller's nitric acid test. It is even better than the latter in a cloudy fluid, as in urine with urates in excess, because the clear water above makes a perfect medium in which to detect the faintest cloud, while the layer of coagulated albumen in Heller's test may be entirely obscured by the opacities in the fluid itself. If no test-tube or nitric acid is at hand, pour boiling water into a common tumbler, let it stand a moment to insure the heating of the bottom of the tumbler, empty, refill, acidulate with vinegar, and proceed as before. While this is a modification of the heat and acid test, it has the advantage of being applicable under all possible circumstances, whether special apparatus is at hand or not. It is as convenient and accurate in the farmhouse as in the laboratory." (British Med. Journal, Feb. 24, p. 356.)

BLADDER.—*Exploration by Finger for Diagnosis of Obscure Vesical Diseases, and Removal of Tumours, Calculi, &c.*—It is necessary that the tip of the index-finger should be placed just within the internal meatus at the neck of the bladder. Next, it is to be remembered that when the bladder is empty —that is, not distended by urine or by a calculus—the cavity presented for exploration is a small flaccid bag, sometimes merely a cul-de-sac, every portion of which is situate at a short distance from the finger placed in the position just described. Lastly, if the finger of the operator's left hand be maintained in that position, while he makes firm suprapubic pressure with his right hand, I maintain that there is no difficulty in bringing every portion of the internal surface of the bladder, successively, into close contact with the tip of the left index-finger; a contact which enables him at once to estimate correctly the condition of the interior, in relation to every important particular necessary to be ascertained. An objection may very naturally arise in some minds—viz., That we are not always able to reach the neck of the bladder with the tip of the finger when the incisions for removing a stone by the lateral operation have been completed, and hence it is notorious that a long and flexible index-finger is an important element in the making of an able lithotomist; yet, although possessed of it, he may still be unable to reach the point in question, in a few exceptional cases. To this I reply, that there are few bladders indeed into the neck of which, even when the prostate is large, a finger of ordinary length may not be introduced, if carried straight in from the centre of the perineum, which is the shortest route from the surface. But, in addition to this, I refer to the fact which I have verified by experience—that firm suprapubic pressure made by the right hand of the operator, or aided by an assistant if necessary, will accomplish the object, even in an exceptionally deep perineum, if the index-finger is firmly pressed up from the perineum to meet the other hand. Of course, when large outgrowth of the prostate is present, occupying the neck and cavity of the bladder, it may be impossible to carry the finger to the point desired; but then it is to be remembered that in such circumstances there is no need to make incisions in order to explore the internal surface of the bladder, the case not being an obscure one, but, on the contrary, quite unmistakable. Let us now consider the manner of operating, so as to place the tip of the left index within the neck of the bladder. The first object to be attained is the opening of the urethra at or about the membranous portion, by the simplest and shortest route from the surface of the perineum, making a passage only sufficing to admit

with ease the entry of the forefinger and no more. There can be no doubt that a vertical median incision—that is, one made in the line of the raphé—will fulfil this indication better than any other. In this situation the prominence which is formed by the bended knuckles of the operator, when introducing as far as possible the index-finger, lies in the hollow equidistant between the two nates, and presses directly towards the centre of the bladder. Any incision made right or left of the median line must of necessity lead obliquely to the centre, and be therefore a longer line, because it commences at a point on the external surface more distant from the neck of the bladder than is the raphé of the perineum. Accordingly I prefer, and always employ, a vertical incision in the centre of the perineum, carried straight to the urethra, aiming at the point close behind the bulb, a procedure which as far as the incisions are concerned has been practised by surgeons for centuries to relieve stricture, retention, &c., objects, however, altogether differing from that for which I have employed it, and which is the subject of our consideration here. The patient then is to be placed in the ordinary position for lithotomy, a rather short, well-curved staff, with deep median groove is passed into the bladder. The operator enters the point of a long, straight bistoury about three-quarters of an inch above the anus, and makes a vertical incision upwards of the skin, not more than an inch or an inch and a quarter in length. He then carries inward the knife deeply, nearly parallel with the rectum, in which the left index should be placed, to inform the operator of the relative positions of the blade and the bowel, until the point reaches the groove of the staff about the membranous portion of the urethra. He is next to incise the tissues covering the groove for about half an inch; the left index is then to be introduced into the wound, the nail insinuated into the groove, then slowly into the urethra beyond, gradually dilating it. When the finger is fairly lodged in the prostatic urethra, the staff is withdrawn, and the finger is pressed firmly onwards until it enters the neck of the bladder; and it should now be felt to fill, or nearly so, the entire wound, and it does so if the incisions have been made as directed. As a rule very little bleeding results. The operator now, maintaining his finger in the situation described, should rise from his seat to the standing position, place his right hand on the patient's abdomen, directly above the pubic symphysis, and make firm pressure into the pelvic cavity. He will soon recognise the end of the left index finger, and by concerted movements of the two hands he may, as I have already said, examine with ease the upper surface of the bladder, and explore the late-

ral walls, the fundus, and trigone; the latter more completely, if necessary, by placing the other index-finger in the rectum. You will observe that the procedure thus described is simply a limited external urethrotomy, not "cystotomy," as when incision of the neck of the bladder has also been made; much less is it "lithotomy" in any form. The incision involves only a small portion of the urethra anterior to the prostate and neck of the bladder, which are left perfect and intact throughout. Hence its result is to enable the male bladder to be examined almost as readily as that of the female in its natural condition; and in the female, as you know, dilatation of the urethra alone suffices to permit the finger to enter the viscus and explore it. We will now suppose the result of such an exploration to have been that the operator's finger recognises the presence of a soft protruding mass or flocculent growth springing from some part of the bladder. If brought within reach of the finger by supra-pubic pressure, it is easy to verify the nature of the peduncle, whether it be narrow or broad, together with any other physical characters which are obvious to the touch. The operator will next introduce a forceps, the extremities of which meet by broad roughened edges, so that they nip off, without cutting, the greater part of the salient portions of the tumour. Those that I have designed for the purpose are shown in Fig. 1, p. 306. The extremity of each blade meets its fellow by a margin of about an inch long and about one-sixteenth of an inch broad, and these are indented so as to effect a separation of the morbid tissue with as little chance of producing hemorrhage as possible. With this instrument the greater portion of almost all growths can be removed. But I soon met with a case in which a small growth protruded close to the neck of the bladder, and although with the forceps described I took away the protruding portions from the other aspects of the bladder, I failed with this instrument to seize the small one close to the neck. For this I designed another forceps (No. 2, Fig. 2). It will be readily seen that by means of this instrument it is easy to remove any growth in the situation described, and both forms should be at hand when making exploration of the bladder. (Sir Henry Thompson, p. 301.)

Bougies.—*Filiform Bougie Lengths.*—At a recent clinical lecture at the Liverpool Royal Infirmary, Mr. Harrison exhibited some flexible filiform bougies which had been made for him by Messrs. Krohne and Sesemann of London. Their use was also demonstrated on a patient with a tight stricture, where considerable difficulty had previously been experienced in getting any instrument into the bladder. The peculiarity of

this form of bougie consists in its being made in lengths of twenty-four inches. At the extremity it corresponds in size with the finest filiform instrument, and gradually increases in diameter for eighteen inches, until No. 8 (French scale) is attained. In explaining its use, Mr. Harrison stated that in the construction of these instruments he had adopted a principle which was well known, but had not, he believed, previously been applied in a precisely similar manner. In cases of tight stricture which only admitted a very fine filiform instrument, it was sometimes found impossible to pass successive sizes, and consequently the process of dilatation was arrested at a stage at which its performance was most difficult to accomplish. By putting eight sizes (French scale) on one instrument this difficulty was surmounted. The transition was very gradual, and as the end of the instrument entered the bladder, it curled up within its interior until the whole of the bougie (as far as the increase in size went) had been made to pass through, and thus gradually to dilate the stricture. In this way eight sizes (French scale) of filiform instruments were comprehended upon the one length. When the urethra had been dilated sufficiently to receive a No. 8 bougie (French), Mr. Harrison considered that the chief difficulty of these cases had been removed; he therefore thought that this adaptation of the wedge principle might be found useful in practice. To make the process sufficiently gradual, the length of these instruments had been extended to twenty-four inches. To distinguish them from ordinary filiform bougies, it was suggested that the term 'filiform bougie-lengths' might be applied to them. (Mr. Reginald Harrison, Lancet, Feb. 3, p. 199.)

Calculus in the Female.—Mr. Heath first dilates the female urethra, perhaps with his little or forefinger, and can then probably feel the stone, and clear out any adherent particle from the inside of the bladder. "Then, through a little vulcanite speculum, I applied a solution of nitrite of silver (a drachm to the ounce) over the whole surface of the interior of the bladder, and sent the patient to bed with a half grain of morphia suppository in the rectum. I expected that there would have been more or less complete incontinence for a day or two, after which the patient would have recovered perfect control; but this was not the case. She was able to hold her water completely on the day of the operation, and passed from the first acid urine, instead of the abominably offensive ammoniacal urine which had been present before. Now, this I find the invariable result of mopping out the bladder with a strong solution of nitrate of silver. Extraction of

small stones per urethram with polypus-forceps or a scoop is readily performed if the urethra is rapidly dilated in the way I have described; but it is never worth while to drag out large calculi at the risk of producing incontinence, when they can be so easily reduced in size by crushing. The method is, however, very satisfactory for the removal of the foreign bodies which occasionally find their way into the female bladder. Various ingenious instruments have been contrived for catching, doubling up, and extracting such articles as hairpins; but I have found practically that with the finger introduced through the urethra there is no difficulty in guiding a pair of polypus-forceps, and extracting a hairpin or other similar body without the risk inseparable from a supra-pubic lithotomy, which has been performed in similar cases." (Mr. Christopher Heath, p. 316.)

Dilatation of Female Urethra.—It is undoubtedly true that the urethra and neck of the bladder is, in some females, susceptible of remarkable dilatation. In an operation for the removal of a soft villous growth from the bladder of a female, Winckle introduced both fingers and forceps into the viscus, the urethral mucous membrane was fissured, and yet none of the 150 grammes of water injected after the operation escaped from the bladder. In a similar operation by Schatz, although the urethra was dilated to an extent admitting two fingers, she could not retain her water one hour after the operation, though she soon regained the full tonicity of her sphincter; but, on the other hand, it is well-known that this *not difficult* operation has been responsible for a great deal of suffering and misery in the protracted, and, in some instances, permanent incontinence of urine which it has occasioned, and rather than inculcate a feeling of safety, the novice should be warned against repeating an accident which has occurred in most experienced hands. That laceration of the urethra occurs, as a matter *of course*, is not to be wondered at when we consider the rude instruments that are made to serve the purpose of urethral dilators. In order to conform to the conditions by which alone a maximum degree of dilatation, without laceration, can be accomplished, the expansive force of the dilator should be distributed equally over all parts of the circumference of the urethra, by the unrolling of rounded blades, and not upon two or three sides of the canal as effected by the uterine dilators, polypus and bone forceps, so commonly employed. Again, the dilatation must be as gradual as it is continuous and uniform; that is, the degree of force exerted should be carefully regulated to the amount of resistance to be overcome. An instrument providing for the conditions aforesaid is constructed by Tiemann and Co. of

this city, and has been used here with great satisfaction for several years. It is both dilator and speculum. When closed it is 39 mm. in circumference and is capable of expansion to 6½ centimetres. (Dr. Alex. W. Stein, Lancet, Jan. 27, p. 168.)

Discharges of Pus from the Male Urethra which are not Gonorrhœal.—Dr. Tom Robinson gives the case of a gouty patient who suffered from a profuse purulent discharge from the urethra, which he believes to be non-venereal. He also refers to enlarged prostate and organic stricture as being common causes for these discharges. He does not doubt that muco-purulent discharges, not distinguishable from gonorrhœa, do occur in instances where the irritation is not set up by specific pus-cells. In our experience a non-venereal purulent discharge from the urethra, occurring where there is no organic disease in the canal, is extremely rare. (Edinburgh Med. Journal, March, p. 849.)

Phimosis.—*New Method of Operating by Circumcision.*—In congenital phimosis, besides the more or less narrow orifice, adhesion between the inner layer of the prepuce and the surface of the glans penis is present in every case. In the ordinary method of circumcision, after the redundant prepuce has been cut off, it is frequently troublesome to separate this inner layer from the tiny glans, wet and slippery with blood. By a slight change in the order of procedure, these adhesions can be got rid of *before* cutting, and also the necessary sutures can be passed before the parts are obscured or rendered slippery with blood, while the preputial mucous lining is less roughly treated than in the usual plan. The last case I operated on, in a child of two years, scarcely admitted the point of a probe through the orifice in the prepuce, but by dilating this orifice forcibly with "sinus forceps," and the addition of a few tiny snips with scissors round the margin of the orifice thus dilated, the foreskin could be drawn back until the point of the glans showed itself. Further retraction was prevented by the adhesions referred to, but these were easily broken down by means of a probe passed between the prepuce and the glans, and this done until the corona glandis was exposed in its whole extent. The prepuce was next replaced forwards, and the amount to be cut off was marked by a clip arrangement made by tying two ordinary directors, groove to groove, at one end and slipping the prepuce into the clip formed by the untied ends. Three carbolised silk threads were then passed through the prepuce at equal intervals close to the clip on its proximal side, the glans being guarded as the needle was passed, and each thread being of sufficient length to form *two* sutures.

The prepuce in front of the clip was then cut close off, the clip separated, the penis released, vessels twisted, the threads fished up with a blunt hook from the now enlarged preputial slit, cut and then tied on each side. The orifice in the inner or mucous layer of the prepuce can then be slit with scissors down to the corona, but this is unnecessary if the clip is put on so that the line of section runs in the direction from the corona to the orifice of the urethra. The surface of the glans being anointed with vaseline, a plug of absorbent cotton dipped in a 1 to 20 solution of boro-glyceride made an excellent dressing, and was kept applied by a bandage passed round the abdomen, knotted behind, and the two ends brought forward between the legs over a piece of light macintosh or oiled silk, the bandage ends diverging so as to include the genitals, then converging and being looped through the bandage crossing the abdomen. The absorbent pad was changed every time that urine was passed. Healing took place by first intention, and not a trace of odour was detected from first to last. Carbolized catgut sutures would have been better than silk, as they do not need to be removed. (Dr. Neil Macleod, Edinburgh Med. Journal, March, p. 807.)

Prostatic Tumours.—*Enucleation of.*—Lateral cystotomy may be practised in certain cases of enlarged prostate which are attended with symptoms producing great distress with the view of exploring and if possible of removing the growth. In all cases of cystotomy for calculus where the prostate is found to be enlarged, a careful search should be made with the finger, with the view of effecting the removal of the growth should such be found practicable. In determining the selection of lithotomy or lithotrity in a case where stone in the bladder is complicated with enlargement of the prostate, regard should be had to the possibility of removing both of these causes of distress by the one operation, namely, by lithotomy. The cases I have recorded seem to indicate the mode in which these growths may best be removed when met with, either in the course of a lithotomy, or a cystotomy performed for the purpose. The presence of isolated growths in the prostate can be ascertained when the gland is opened into, by exploration with the finger; for, as Sir W. Fergusson observed, "as the finger passes towards the bladder, the sensation is as if its point glided through several rounded bodies in the substance of the gland, which are but slenderly in contact with each other." Of this sensation I have been conscious in more than one lithotomy I have performed in elderly persons. Thus discovered these growths may then be enucleated by the finger as I have already endeavoured to demonstrate. (Mr. Reginald Harrison, p. 308.)

Renal Calculi and Gravel.—*Influence of Diet.*—The occurrence of gravel and calculus is largely influenced by the diet. I feel sure that the opinions frequently held are not altogether correct, and require to be reconsidered. A gouty diathesis is so potent in the production of the diseases under review, that it will be quite safe to assume that what tends to produce gout tends also to develop calculus, and that the diet which is of avail in the treatment of the one disease is equally so in the management of the other. It will be desirable to turn our attention to the principal groups of aliments and ascertain what influence they have upon the formation of uric acid and also upon its condition with respect to solubility:—

Sugar.—The most common of the non-nitrogenised principles contained in food is starch, seeing that it forms 70 per cent. of wheaten flour, and almost the whole of many of the simple amylaceous articles of food, as rice, maize, arrowroot, sago, &c.; also of the potato, turnip, carrot, and so on, when these latter are dried. When taken into the alimentary canal, starch is soon changed into glucose sugar by the action of the saliva and pancreatic juices; and, when cane sugar is taken, the same change ensues, so that however carefully sugar is avoided as an article of food, it is still abundantly formed in the canal when amylaceous matters are eaten, and the result is the same whether a pound of starch in any of its dietetic forms or a pound of cane sugar be taken. There is a very popular idea that sugar causes what is termed acidity, and hence it is scrupulously avoided by many. Is this true? Between two or three years ago I was much struck at seeing an American surgeon of great repute putting lump after lump of white sugar into his tea, and I asked him why he did so. He told me that in the States it is a common habit to take sugar thus as a preventive of heartburn, and that he took it for that purpose. His answer made a strong impression on my mind, and since then I have often questioned dyspeptic patients as to their experience on this point. At first nearly all exclaim, "Of course sugar causes acidity," but as yet I have failed to find anyone who could assure me, from personal experience, that the eating of lumps of ordinary white sugar produces more so-called acidity than taking any other article of diet. It must be borne in mind that I do not for a moment include sweetened fruits, and such-like substances, in the same category as simple sugar. One can hardly believe that the eating of a lump of cane sugar would seriously add to the glucose which is daily produced in the alimentary canal of an individual living on an ordinary mixed diet. Let us see what has been found experimentally with regard to the influence

of sugar on the production of uric acid. Böcker says that the effect, in man, is to lessen the quantity of that principle, and Bischoff and Voit have proved that, in dogs, starch produces the same effect on the urinary excretion as sugar, so I think we may say that there is no increase in the uric acid when sugar is taken. (Dr. Garrod, p. 218.)

Alcohol, Wines, and Malt Liquors.—It may, as I believe, be confidently asserted with respect to gout, that, with an absence of alcohol in any shape, coupled with an absence of hereditary predisposition derived from alcohol-drinking ancestors, the disease would be practically unknown. It is most essential to separate the different kinds of alcoholic beverages from each other in estimating their tendency to produce disease. Thus, alcohol in the form of distilled spirits, although when taken in excess it causes serious mischief, injuring the liver, kidneys, heart and other organs, still has little or no power of producing the uric acid diathesis, or, at any rate, the gouty development of it. In spirit-drinking countries, or among spirit drinking families, gout is unknown. Look at Scotland and its whisky-drinking classes —the disease is practically absent, hardly ever seen in the hospitals. Look at Poland, where they drink a kind of arrack; the same holds good. When, however, we investigate the influence of wines we shall find a different result. Drinkers of the common light wines, such as the red Bordeaux and the Rhine wines, suffer but little, while, among the same nations, those who indulge freely in beer are by no means free. The natural light wines, in which the alcohol is small in amount, while there is an almost complete absence of unfermented matter, which contain, also, a considerable amount of acid vegetable salts, are little liable either to produce gout or to lead to the formation of calculus or gravel. On the other hand, the Peninsular wines and those which resemble them, which are stronger in alcohol, contain much unfermented matter, and are almost devoid of the vegetable salts, have great gout-producing power, and lead readily to a condition of urine favourable to the production of gravel and calculus. We come, lastly, to the malt liquors, ale, beer, stout, and porter. From my own experience, and I believe it is also the experience of all who have attended to the subject, I can confidently assert that these beverages have a great tendency to produce the uric acid diathesis. Compare the hospitals of Edinburgh and Glasgow with those of London; in the former gout is scarcely known, in the latter the disease is common, the difference, as I believe, being chiefly due to the different beverages drunk by the working classes of the two countries; it is, in fact, the difference between whisky and malt liquors.

It is necessary that we should at least endeavour to ascertain what principle or principles, present in some of these alcoholic beverages, absent from others, lead to the development of this diathesis or aggravate it when it is already manifested owing to hereditary or other causes. It cannot be the alcohol alone; this, I believe, can be fully and satisfactorily proved, seeing that large groups of people whose custom is to drink freely of distilled spirits are yet free; instances are to hand in Scotland, Sweden and Norway, and Poland. It cannot be the sugar alone; for, although the partially fermented wines and malt liquors contain sugar, yet sugar added to distilled spirit does not appear to produce the uric acid diathesis. It cannot be the acidity alone; for the wines which are most harmless are quite as acid or even more so than malt liquors and the Peninsular wines, and many people who strongly object to the least acidity in wines, will often take lemon juice to an extravagant extent. If, then, neither the alcohol, nor the sugar, nor the acidity by itself is the cause of certain beverages proving so injurious, is it a combination of any of these that does the harm? We already know that the combinations of alcohol with sugar, and that of alcohol with acid salts, are innocuous as far as the uric acid diathesis is concerned. What, then, is there left for us to fall back upon in explanation of the peculiar properties which some of these beverages possess, while others are devoid of them? The only conclusion that I can arrive at with my present knowledge—and it is the result of much thought during many years—is that it is something which is a result of imperfect fermentation, and you will find that it is those beverages in which fermentation has commenced, and has been allowed to proceed to a certain extent and has then been checked, which, of a certainty, cause gout, and probably lead also to the production of gravel and calculus. If I am asked to state more exactly what this principle is, I cannot do so; it may be an influence only, a condition of matter—a ferment. (Ibid, p. 218.)

Sweets.—In connection with this subject, however, I must return for a moment to that of sugar, which, I told you, had, as I thought, been regarded askance without due cause. I would say that I do not for a moment classify with sugar either sweetened fruits or vegetables; for I am quite sure that such articles of diet will frequently produce heartburn and other dyspeptic annoyances in individuals who are not in the least inconvenienced by sugar itself. I cannot help thinking that these contain a something which is not simple sugar, but a substance which is the result of the long contact of the sugar with the fruit or vegetable juices, a kind of semi-fermented

matter—in fact, that same 'something' which exists in the stronger wines and the various malt liquors. Of this I feel confident, that in many cases where sugar, whether by itself or in tea, coffee, and light puddings, does not disagree, and where fresh fruit, although sweet, produces no discomfort, the combination of sugar with these juices, if time has been given for them to act upon each other, will often cause well-marked dyspeptic symptoms. But, it may be said, if so, a ripe orange cannot be a good thing to eat, as it contains both sugar and acid juice, and these substances have been in contact with each other for a long time. I answer, not necessarily so; so long as the orange exists as a fruit, with its botanical structure intact. so long there may be no change taking place between its different constituents. We have a striking analogy to this in the case of the bitter almond. When whole this seed contains the crystalline amygdaline and an albuminous ferment. Separate one of these from the other, and each by itself is innocuous; crush and moisten the almond, prussic acid is immediately formed, and the union of the two principles is the production of a deadly poison. (Ibid, p. 220.)

Animal Food.—As a result of all the experiments made by different observers, both on man and the lower animals, we may fairly conclude that meat, taken in such quantities only as are sufficient to keep up the nutrition of the body, has no tendency to increase the excretion of uric acid; that, when the diet is purely animal, but the quantity small, the uric acid, far from being large, becomes exceedingly small in amount, more especially when it is compared with the urea. On the other hand, the taking of a great quantity of meat—an excess compared with the requirements of the system—tends to increase the uric acid, though, even then, not more than in proportion to the urea. These different facts can be advantageously applied in practice in the treatment of gravel and calculus. In such cases, there is certainly no reason why a proper quantity of animal food should not be taken; and the knowledge of this is important, seeing that many patients have been lowered in health by being kept on insufficient diet, with the idea that by these means a lessening of the excretion of uric acid would result. (Ibid, p. 221.)

Effects of Alkaline Treatment on Uric Gravel and Calculus.—*Lithia.*—If we look to the atomic weight of the metal lithium we find it very low, only 7. The number representing the carbonate of lithium is also small; compared with carbonate of potassium it is as 74 to 138; hence the neutralising power for acids possessed by carbonate of lithium is greater than that of carbonate of potassium in the above proportions.

The acid urate of lithium requires only 220 parts of water at the body temperature to dissolve it; the corresponding potash salt requiring 500 parts, and the soda salt as much as 1130 parts, while the magnesia and lime urates take 1600 and 2800 parts respectively; so that, with respect both to neutralising power and solubility the lithia carbonate has a great advantage over the corresponding salts of potash, soda, magnesia, and lime. In a paper in the Medico-Chir. Trans. 1875, on the 'Solvent Treatment of Urinary Calculi,' Dr. W. Roberts, of Manchester, came to the conclusion that potash carbonate dissolves uric acid more rapidly than the soda salt. This he ascertained by placing sections of uric acid calculi in phials, and causing currents of the different solutions, at blood heat, to pass over them at a regulated rate. He also found that the strength of the solution employed was of much importance, the greatest amount of solvent power being exhibited in solutions containing from forty to sixty grains of the alkaline carbonate to the imperial pint (20 fluid ounces). Below this strength the power of the solutions gradually declined, until, with those which contained less than three grains to the pint, the solvent power scarcely exceeded that of water. On the other hand, if the strength was above sixty grains to the pint, the pieces of the calculus became encrusted with the alkaline bi-urates which were then deposited, and thus the further action of the solution was impeded. This was especially noticeable when the strength of the solutions was much above 100 grains to the pint. Even without the actual experiment with pieces of uric calculi, I think we could predict that potash would prove a more powerful solvent than soda; for we have only to glance at the table to see that urate of potassium requires, at the body temperature, only 500 parts of water to dissolve it, whereas the corresponding soda salt takes as much as 1130 parts. The soda salt certainly has an advantage in its greater neutralising power, but not sufficient to make up for the far less solubility of the resulting urate. In the paper referred to, and in his book on 'Urinary and Renal Diseases,' Dr. Roberts has the following footnote: "Some experiments were also made with carbonate of lithia, which has been vaunted in recent times as a solvent for uric acid. Its power was found much inferior to that of carbonate of potash and soda. Its reputation seems to have been gained through its comparative insolubility. Only weak solutions of it could be employed." I refer to this passage because other authors have evidently been influenced by the statement; for example, Sir Henry Thompson, in his little work on the 'Preventive Treatment of Calculus Disease,' says: "Dr. Roberts finds

carbonate of potash to be the most powerful solvent; better than soda, much better than lithia." But I think that lithia salts are far more powerful solvents of uric acid than potash salts, while these latter are more efficacious than those of soda. We have only to take three small phials, filled with a solution of the three carbonates, of the same strength, and to put into each the same quantity of small uric calculi, the amount being such that the lithia will dissolve them. If we carry these in a warm pocket, after a short time it will be seen that all the calculi have disappeared from the lithia solution, while more than half are left undissolved by the potash, and about four-fifths by the soda solution. (Dr. A. B. Garrod, p. 224.)

Renal Inadequacy.—There are cases which, in some measure, I venture to separate off entirely from Bright's disease, in which the kidney, without any sensible alteration of structure that our modern means of investigation will enable us to determine, cannot produce a healthy urine. Such kidneys produce a urine which, assuming the quantity to be a quantity of health, is low in density, and is deficient in solid constituents, principally the constituent of urea and its congeners. I will exclude uric acid. I have spoken on this subject once before, and, on that occasion, I committed an error. I said that the main feature of a kidney in this state was, that it could not secrete urine with a proper amount of urea and uric acid; but I find that many of these kidneys, of which I shall speak more precisely in a moment, do not have a sufficiency of uric acid; I have learned that since. They have all, however, a deficiency of urea. There is a certain state of the kidney, I repeat, in which, without any alteration of structure that the eye can detect, it can, nevertheless, not produce a perfectly healthy urine. It is an urine low in density and deficient in solid constituent, principally in urea and its congeners. I call this state renal inadequacy. Some years ago Sir James Paget said, "Whenever I find a man in ill-health, without definite cause for the ill-health, I feel sure that my chances of success in operating upon him are diminished by at least one-half." Subsequently he found out, and recently expressed the opinion to me which I had expressed upon a patient of his, that it was due to the low density of the water. The story is this. I was summoned to see a man who was about to undergo a surgical operation, and I was asked the question, "Is this man a suitable person for a surgical operation?" I required, before pronouncing judgment, that I should have the complete urine of two days kept and sent to me. I examined it. The urine was under the quantity—under forty ounces; the density was 1008, or

thereabouts: there was no albumen; there were no casts; but the urea was 1.2. I replied, that it would be at the peril of his life if any operation were performed upon him. They were not satisfied—at least the surgeon was not—with this expression of opinion. Sir James Paget was called in, and he gave the same judgment. Meeting him afterwards in consultation, he asked me the question, "Why did you object to an operation being performed upon the patient?" I said, "On account of the state of the urine;" and he said, "I objected also on the same ground." That shows that from different quarters this question of renal inadequacy has presented itself to the surgeon's mind as well as to the physician's. This, then, is what I mean by renal inadequacy. I refer to those people who have kidneys which, though not materially altered, or at least not altered in any way that we can determine by physical investigation, are yet incapable of producing a sufficiently healthy urine (like an imperfect skin, that is incapable of producing sweat); I mean an urine sufficiently rich in the ordinary matters of waste which it is the business of the kidney to discharge from the body. When you get hold of a patient who is ill, suffering sometimes from dyspepsia, or nervousness, having headaches or complaining of *malaise* and weakness, who cannot sleep well, who cannot do his work very well, examine his urine, and if you find that the urine is low in density, you had better proceed a little further, and be very precise, and get the urine of twenty-four hours; and if you find that it is under fifty ounces in quantity, that it has not a specific gravity of 1010, and that the urea in it is deficient in amount—under 2 per cent.—then, whether there be albumen in the urine or not (I am not now speaking of albuminous urine), whether there be any casts or not, whether there be granular *débris* deposited or not, you may know with certainty that the kidney is not doing its duty. Well, it may not be, although these kidneys are not doing their duty, that the defect is the cause of the patient's ill-health. How are you to ascertain that? You can sometimes, not always (always does not happen to honest men), ascertain it in this way. If you give a patient a liberal diet he gets worse; and what is the strangest thing I know about these cases is that, if you give a patient a very liberal diet, namely, food and wine, the specific gravity of the urine, instead of increasing, as you would expect, diminishes in density. Furthermore, you may discover that the renal trouble is the cause of their symptoms if you notice in them—and you can notice it very often—that a diminution in the bulk of the urine is always attended by an aggravation of their sufferings. And there is one other way, which is the other surprising

thing about these cases, that if you diminish the quantity of food, if you make the patients careful about their ingoings and careful about their outgoings, you will see at once a great improvement. I have, for example, a case in my mind's eye, which I saw with two or three doctors some time ago. The patient was a man who was extremely distressed, short of breath, with palpitations, headaches, and great distress of body in various ways; he had a weak heart also. The doctors who had seen him said that he wanted keeping up. Being a rich man, he was kept up, and he was no better for the keeping up. He had meat and wine in plenty, but he got weaker and weaker. He was then put upon a starving plan—that means, upon a physiological plan, upon a plan which would give him just enough good food for the maintenance of the body and no superfluity, with enough water to form an abundant vehicle for the chemical operations which are necessary, not only for the formation of waste matters, but for carrying them off. He was put upon an old woman's diet, three meals a day; tea and toast for breakfast, a midday dinner, with a little animal food, and tea and toast for tea—a John Abernethyan diet. In a very short time the urine, which had gone down to 1004, began to rise; instead of getting lower and lower in density with the diminished diet, it got higher and higher, and the patient got better and better—not quite well, but pretty well. These circumstances with reference to the state of the urine, the diminution of the urea, the fact that the increase of food makes the patient worse, and that, within certain limits, the diminution of food makes him better, with attention to the secretions—these circumstances put together will soon enable you, I think, with sufficient accuracy to determine what is the sort of case that you are dealing with; and I venture to think that the knowledge which you will acquire as you go on will be sufficiently useful to you to justify me in having presumed to occupy your time for a few moments to-night. You will know one or two things about these patients which, for their safety and their well-being, it is well to know. You will know that these people cannot be operated upon with the ordinary chances of success. You will know that if they take cold you must treat the cold in them as a serious thing. You will know that if they are ill, that if they have a headache or pneumonia, you will be able to warn the patient's friends (and foresight is an important thing in medicine) that they may not recover immediately from the effects of the disease—nay, more, that the unabsorbed pneumonic products may remain to be centres and sources of future mischief. These are important points. (Dr. Andrew Clark, p. 209.)

Retention of Urine from Prostatic Disease.—It occurred to me, that were it possible to suspend all action on the part of the bladder for a few days only, to prevent any accumulation of urine with the organ, to allay the constant and painful want to pass urine, and also at the same time to abolish catheterism altogether, with its irritating effect on the urethra, the inflammation of the bladder might subside, and its tolerance of urine might considerably increase. And I hoped that a state of things might be subsequently brought about, similar to that which is present in a less aggravated stage of obstruction, when catheterism is not needed more than six or seven times in the twenty-four hours. If, in place of hourly relief by the instrument, an interval of three or four hours could be made to suffice, an enormous boon would be conferred on the patient. This might probably be attained by a proceeding of the following kind. First, placing the patient in the lithotomy position, under ether, I proposed to pass a grooved median staff into the bladder, and make, from the raphé of the perinæum, a small vertical incision just above the anus, large enough only to admit the index-finger—the incision to terminate in the staff at the membranous portion of the urethra, which should be divided for half an inch at most, so as to admit the finger to traverse the canal to the neck of the bladder. Then, having withdrawn the staff, I proposed to insert a large vulcanised catheter or tube, say about No. 20 (English scale), with its extremity just within the bladder, fastening it there by tape to a bandage round the waist; the tube to be retained as a channel for the urine, for several days at least. An opportunity soon offered of making an opening in the manner described, for a patient at the age of sixty, who was passing the catheter every hour, and whose vital powers were at the lowest ebb from constant suffering and loss of rest, but whose constitution was apparently sound; and I placed in the bladder, by the new passage, an India-rubber catheter, so that the urine might flow off continuously into a receptacle as fast as it arrived in the bladder. The relief was immediate, and most remarkable. He enjoyed long periods of unbroken sleep, and was unconscious of any pain; while the urine itself, which had been charged with muco-pus and blood, and had been alkaline and offensive in the highest degree, assumed in the course of a few hours a healthy colour, an acid reaction, and was almost clear. In two or three days, the patient had regained appetite and digestion, became cheerful, and showed a change for the better, which no one had been sanguine enough to anticipate. On the eighth day, I removed the catheter from the wound; during the next two days,

urine issued by that route at intervals of some hours; but the wound, which was very small, rapidly closed, and the catheter was, of course, again necessary. But the passing of the instrument was no longer painful; the bladder was not inflamed, and could now retain urine three or four hours without inconvenience; while the patient himself, in less than three weeks from the operation, was enjoying an active life out of doors, having been long confined to his room in the suffering condition which has been described in general terms above. (Sir Henry Thompson, p. 297.)

When complete retention of urine from enlarged prostate occurs, it frequently happens that the introduction of an instrument is followed by temporary return of power to micturate; and in other cases of partial retention it is well known that the occasional passage of an instrument will for a while restore the ability to empty the bladder almost completely without help. The cause of the difficulty being a mechanical one, I suppose there can be little doubt that the introduction of an instrument does good in this way by pressing aside that portion of the enlarged prostate which is most immediately concerned in producing the obstruction. Now, much more good in this direction, and good, too, which will last much longer, is often gained by retaining a catheter for some time after it has been introduced—say for one or two hours or so, as the patient may be able to bear it without distress. This plan is well worth trying in most cases of the kind. When an instrument has been passed, whatever difficulty there may have been in its introduction has been overcome, and the patient is subjected to little or no additional trouble by its retention for a short period. I may add that for this purpose a silver catheter appears to me to be of more service than a flexible one. It will be observed that this plan of repeatedly retaining an instrument for an hour or so after it has been passed is quite distinct in principle and purpose from the practice which has been advised, and is sometimes adopted, in cases of complete retention, or of very frequent micturition, or where there is unusual difficulty in the introduction of an instrument, of retaining it for many hours or even days together. The object here is either to escape a difficulty which may become insuperable or to avoid the necessity of passing an instrument so frequently as to make this a source of grave irritation and further mischief. Here, unfortunately, the proposed remedy is often worse than the evil. The plan now advocated has been suggested with the view of taking advantage of the passage of a catheter, when it is required to relieve the bladder, to retain it for its

effect upon the prostatic portion of the urethra; for the good it does in this way of restoring or improving the power of micturition, or possibly by pressure promoting in some degree absorption. In speaking on this subject, I would add that in my experience, as in that of others, in cases even where the prostate is considerably enlarged, it is often easier to introduce a catheter with an ordinary curve than the instrument which is especially made for cases of this description. Surgeons know very well that sometimes when a prostatic catheter cannot easily be passed, an instrument with a much smaller curve will easily slip in. I fancy that the advantage on the side of the smaller instrument is more common than it is generally supposed to be. With me, at least, it is the rule; and so, to relieve the bladder in cases of enlarged prostate, I should take first an instrument of full size with an ordinary curve, or a curve not exceeding the quadrant of a circle two inches or so in diameter. (Mr. W. S. Savory, Lancet, March 3, p. 356.)

Stricture of the Urethra.—It may be well from time to time to call attention to the still prevalent practice of treating all cases of stricture of the urethra by the introduction of instruments. I say all cases; for when a stricture has been once made out, does it ever escape bougies? How many cases occur of contraction due to spasm or to temporary thickening of the membrane from inflammation or congestion, which if left alone would speedily clear up, but which are worried into permanent stricture by such mischievous activity? Even when organic or permanent stricture is established, how very seldom does it come under the notice of the surgeon uncomplicated by inflammation or congestion and spasm? and are these conditions to be subdued by the employment of instruments? Is this in accordance with any recognised principle in surgery? Many cases of so-called stricture need no instrumental interference from first to last, and by such means are only made worse; and of those cases in which much may be done at the proper stage by the judicious use of instruments, there are very few indeed which do not require, for some time previously and simultaneously, treatment in the way of rest and other measures to subdue the active mischief, which in ordinary circumstances is almost invariably associated in some degree with passive structural contraction. The story is well known of a physician who, having in consultation discovered pericarditis, consoled his colleague who had overlooked it with the reflection, "My dear fellow, if you had found it out you would have treated it." How far the ordinary treatment would have been bad for the pericarditis I cannot say; but

I am sure that stricture of the urethra sometimes suffers very much at the hands of surgery from being found out; and, unfortunately, it has but little chance of escape in the way of being overlooked. In every case, of course, instruments must be passed, and the existing evil is not only apt to be thus aggravated when present, but too often it is nursed into existence. (Mr. W. S. Savory, Lancet, March 3, p. 357).

FRACTURES, DISLOCATIONS AND DISEASES OF THE BONES, JOINTS, &c.

DISLOCATION OF HIP-JOINT.—The principles which should govern the surgeon in his efforts at reduction can be thus formulated. 1. Place the limb in the position it occupied the moment it forced its way through the capsule, thus carrying the head of the femur opposite the opening through which it emerged. 2. Manipulate the limb in such a manner as to relax the untorn portion of the capsular ligament. 3. Draw or push, elevate or depress, the head of the femur in such a manner as to carry it over the brim of the acetabulum, exercising this force by proper movements of the extremity, directed by the grasp the surgeon has on the leg, at the same time so moving the limb as to keep constantly relaxed the untorn portion of the capsular ligament. (Dr. R. A. Vance, p. 248.)

FRACTURED CLAVICLE.—*New Treatment.*—For the past three years, I have used a plaster-of-Paris splint in the treatment of fractured clavicle. The splint extends from below the spine of the scapula behind, to nearly the nipple-line in front, having a concave border on its inner side for the neck, and outside slightly overlapping the acromion process, thus covering the entire shoulders. I make it by sewing the edges of two layers of flannel, which on the patient cover the area described, then slitting the outer layer from the acromion process directly inwards; thus forming two pouches, one behind, the other in front, into which the semi-fluid plaster-of-Paris can be poured. I now place the arm in whatever position the fragments best lie, pour in the plaster, and manipulate it until it is an inch thick all over. I then tie the four elastic strings which are attached to the angles of the splint, two and two tightly together under their corresponding armpits; and, while the plaster is still setting, I bandage the hand across the chest, as in the ordinary manner for treating fractured clavicle, rolling the bandage repeatedly over the splint. Thus a cast of the unbroken bone is made. The weight of the splint prevents either fragment from rising over the level of the other, so obviating deformity. By this

splint I have saved an amount of trouble, one bandaging only being necessary, and I have got union in which there was not the slightest deformity. The splint acts best for fractures of the outer half of the clavicle. (Dr. M. R. O'Connor, Limerick, British Med. Journal, March 3, p. 406.)

HIP-JOINT DISEASE.—*Rest.*—To ensure the most perfect rest, the patient should be kept in bed, and not only kept in bed, but secured to it. It is advisable to confine the sound limb as well as the affected one. This is done at some hospitals by the use of the 'box-splint,' or it may be effected by Thomas's double splint. It is of importance to attend to the structure of the bed, and other details of treatment. The success of a case often depends upon attention to these, apparently trivial, matters. The bed should be firm and flat, the head kept low, and the trunk immovable. I cannot do better than describe the excellent plan carried out at the Alexandra Hospital for the treatment of Hip Disease in Children; there, the bed consists of a single plank, upon which lies a hard mattress; the pillow is just large enough for the head, about a foot square and flat. To secure the patient's body to the bed, an armlet is passed over each shoulder, the two armlets are fastened behind to a strap which passes across the back and is secured to the bed underneath. In front, the armlets are also connected by a band across the chest. This arrangement renders the patient immovable in bed, and doubtless contributes materially to the good results which seem to follow the treatment. (Dr. Clippingdale.)

Rest with Extension.—According to Sayre, this principle was first recognised and carried into practice by Dr. Harris, of Philadelphia, in 1825. Dr. Harris seems to have used the long splint with extending and counter-extending bands. The mode in general use, and deservedly the favourite mode, is that by weight and pulley, first employed by Sir B. Brodie. In this method the trunk is fixed in some way so as to produce counter-extension, and extension is made by a weight attached to the limb. In the application of the weight and pulley it is to be borne in mind that, by a law of mechanics, the weight is diminished by one-half by the passage of the cord over the pulley, so that if the weight be four pounds, the actual tension upon the leg is two pounds. Care should be taken to use no unnecessary amount of weight; only sufficient should be used to overcome the resistance of the muscles. Instead of employing the weight and pulley, Mr. Barwell uses a splint designed by himself. Briefly, this apparatus may be described as consisting of a long splint with two pullies, one at each end. The cords from these pul-

lies are connected together by an accumulator placed upon the outer side of the splint, so that by this mechanism extension and counter-extension are both manifestations of the same force. (Dr. Clippingdale.)

Rest with Extension and Motion.—In this method of treatment the object aimed at is motion of the joint without friction of its surfaces. This plan originated and is principally carried out in the United States. The idea seems to have been conceived by Dr. Davis, of New York, and first carried into effect by him in the year 1855. The apparatus he used is known as 'Davis's splint,' but there have been important modifications of it by Sayre, Taylor, Washbourn, and J. C. Hutchinson, all American surgeons. This method, which may with propriety be called the 'American method,' finds most favour in the country of its birth. Either from want of faith, or want of experience, the plan has never been adopted, although it has been tried repeatedly, in this country. (Dr. S. D. Clippingdale, p. 244.)

Joint Affections.—*Electricity.*—Electrotherapy is only efficacious in cases of chronic arthritis, and is contra-indicated when the acute and subacute phenomena have not disappeared. In the joint-affections of progressive chronic articular rheumatism, the results are but small; and when they are successful, they may be rather attributed to rest than to electrotherapy. In chronic articular rheumatism with uncertain localisation and progress, and especially in the chronic forms of arthritis, consecutive on blennorrhagia, the puerperal state, or injury, more satisfactory results are obtained. The operation consists in fixing the positive pole of a continuous current battery, with from twenty to forty elements, either on the sides or on the upper or lower portion of the swollen joint, and moving about the pad representing the negative pile over the cutaneous surface. The skin becomes red and sensitive where the pad is applied. In the successful cases, the lesions were situated especially on the tissues surrounding the joint, which were indurated and resistant. There were neither fungosities, nor osseous lesions. This clinical fact explains why this treatment does not yield favourable results in gout and in chronic articular rheumatism. It is, therefore, especially in affections of joints, produced by wounds, by the puerperal state, or by blennorrhagia, but only after the disappearance of all the acute symptoms, that the continuous current may bring on either a complete cure, or at least a rapid improvement of the circumarticular changes and the restoration of movement. (Dr. A. Joffroy, Medical Record, Nov. 15, p. 455.)

Spina Bifida.—*Treated by Injection.*—June 1st: The sac was aspirated, and six drachms of clear fluid removed; remains about three-fourths full. No ill effects followed. 3rd: The sac refilled; it was again aspirated and one ounce and a half of fluid removed.—6th: The patient is very unwell. Starts at slight stimuli. Does not sleep, but cries continually.—14th: Is again in usual health, sac aspirated and six drachms removed. No ill effects.—21st: Sac refilled. Half emptied and twenty minims of Morton's iodo-glycerine solution injected.—22nd: No bad symptoms.—23rd: To be out-patient.—27th: Sac much smaller, its walls thicker and redder. The sore has healed.—28th: Tumour has not shrunk for the last day or two; half a drachm of solution injected and collodion freely applied. From this date the tumour rapidly shrank, until the skin over the fissure was level with the rest of the back. The treatment of the talipes has since been proceeded with, and the necessary tenotomies, &c., have produced no bad effects. Simple evacuation of fluid was first tried as recommended by Dr. Morton. It would, perhaps, have been as well to have proceeded to inject at first. (Mr. E. Muirhead Little, Lancet, Jan. 20, p. 96.)

Talipes.—In the treatment of the various kinds of talipes, the cutting of the offending tendons, coupled with the after wearing of steel boots or other mechanical appliances, is the only curative measure followed by many surgeons. The division of the tendons no doubt aids in the rectifying of the mal-position, but unless the foot can be subsequently kept in its proper place tenotomy is of little use, and the old habit speedily regains the ascendency. Theoretically, the wearing of steel boots may appear easy and efficacious, but practically they often give rise to much discomfort, and as the pressure which they exercise is confined to a somewhat limited area, abrasion, and if neglected, even sloughing may take place in consequence. The application of splints is frequently unsatisfactory, as the constant movements of the child soon render the whole apparatus loose, and consequently ineffectual for accomplishing the object we have in view. The introduction of paraffin has, however, been turned to good account here, and many cases of club foot can be cured without division of the tendons by the wearing of a boot of this material for a sufficient length of time. I have frequently practised the method recommended by Macewen, and with excellent results. It may be described as follows;—The patient having been put under the influence of chloroform, a gauze bandage is wound round the limb, after which a sheet of the ordinary coarse wadding of the requisite size, previously

soaked in liquid paraffin, is moulded over it. The foot is now forced into as favourable a position as possible, and kept so until the process of solidifying has taken place, which may be hastened by the action of cold water. The boot can be worn for six weeks, when it may be taken off, and at the end of this period, if the foot should still tend to revert to something of its old habit, the paraffin can be re-applied. It may be fairly claimed for paraffin, in the treatment of talipes, that while thoroughly effectual, it is easy of manipulation, and almost always well borne by the patient. After being applied to the limb it is not liable to subsequent displacement, so that the foot, when once securely fixed, cannot resile to its original position. Movement on the part of the child does not matter, and as the pressure which the boot exercises is equally diffused, it seldom happens that any injury is done to the skin. I have invariably found that age is a most important factor as regards the after result, and the younger the child the more hopeful is the prognosis. When patients are allowed to walk before being operated on, a partial cure is in many instances only to be looked for, and much time and pains are demanded in securing even this. In obstinate cases of talipes varus, the removal of a wedge from the astragalus and os calcis is frequently followed by improvement, and it is not difficult to see that the gap which is so produced must permit of the foot being everted into a more favourable position than it could otherwise occupy. (Dr. James Whitson, Glasgow Med. Journal, Jan. p. 15.)

Wounds.—For the suppression of hemorrhage ligature of vessels with catgut, and torsion, are the simplest means at our command. Sometimes, specially if a wound be over bone, a pad of absorbent cotton-wool, applied for twenty-four hours, will stop oozing. The next stage is the bringing of the sides of the wound into complete contact. For this purpose, I am in the habit of employing one of two methods—suture or pressure. The sutures I prefer are silver wire, hair, or common sewing needles. I believe it to be an advantage to use very fine sutures: and, for small wounds, hair answers most admirably; it is very strong, unirritating, and easily procured. A very excellent way of closing scalp-wounds is to push a sewing needle through the two edges, and bring them together with a thread in figure-of-eight. In using sutures of all kinds, it is of great consequence to pass them through the tissues beyond the wound, whenever this can be done. Thus, with hare-lip, after paring, I pass needles right through the whole thickness of the lip and mucous membrane, keeping them (the needles) entirely from contact with the

wound. I have sometimes seen suppuration commence at a suture, and spread from it, breaking up a wound which was promising to heal well. This risk can be avoided by keeping the suture away from the cut surface. Occasionally, a wound is so placed that a pad will close it throughout. Absorbent cotton-wool is the nicest material for this purpose. If it come near the edges, it is an advantage to have it impregnated with an antiseptic. When a wound of the class I am speaking of is efficiently closed, and all hemorrhage has stopped, I look on any dressing not only as superfluous, but generally as injurious. Simple exposure to the air, keeping the wound dry, cool, and, above all, at rest, are all that is needed. We all know that heat and moisture promote putrefaction; and they should, therefore, be avoided. Sticking plaster is, of all known ways of keeping a wound together, the worst, almost invariably producing suppuration of the parts with which it is in contact. (Dr. R. Maclaren, p. 228.)

'*Dry Dressing*' *Treatment of Wounds.*—Fresh wounds without loss of substance are particularly suited for this plan of treatment. They should be put up without water, the edges accurately in contact; always bearing in mind the necessity of providing for drainage outwards of any effused fluid. Under absorbent pads and elastic pressure, with absolute rest and attention to position, the vast majority of fresh wounds heal rapidly, solidly, and painlessly. When the dressing is changed, which it should only be infrequently, no water should be employed; but if there be any discharge and necessity for cleaning, this can best be done with a pledget of dry lint or of absorbent gauze and cotton; all manipulations to be of the lightest. Such dry dressing simulates the natural scabbing process, but is really more perfect. Wounds of many inches in length heal so directly and perfectly under dry dressing and elastic pressure, that in the course of a few days it is often difficult to detect the fine linear scar on the dry and shrivelled skin. If a fresh wound be attended with loss of substance, some boroglycerine should be poured on the part before application; it prevents too close adhesiveness, and possible bleeding, when the dressing is removed, and has the further advantage of preventing decomposition. The necessary employment of sutures and adhesive plasters, according to requirements, need not be dwelt upon, and I shall only briefly remark that instead of, or in addition to, such bonds of union, I frequently employ styptic colloid, compound tincture of benzoin, or collodion. In wounds with large loss of substance, if healing be slow, action may profitably be stimulated by a variety of the well-known astringent

applications in ointment or lotion, than which I do not know a better than the old red lotion (a solution of one to two grains of sulphate of zinc, in water, with the addition of some compound tincture of lavender) with a liberal addition of glycerine. Position, rest, and pressure remain cardinal indications, poultices and water prohibited. By this I mean stagnant water in the shape of water dressing, which is nearly as potent as a poultice in promoting suppuration and decomposition. It is otherwise with cold-water irrigation, which is consistent with, nay may be made conducive to, perfect drainage, and by its astringent and sedative action produces effects very similar to those of rest and pressure. Cold irrigation is not easy to apply continuously comfortably, and one of its great advantages, the low temperature, may be secured by ice bags. (Mr. Sampson Gamgee, p. 242.)

Drainage of Wounds.—The drainage of wounds is now recognised as one of the most important elements in their successful treatment, for if the discharge does not get a free exit tension must follow, which in its turn gives rise to inflammation, and if these remain unrelieved for a sufficient length of time suppuration is the inevitable result. The india-rubber drainage tubes of Chassaignac were the first agents employed for this purpose. The material of which they are composed is non-porous, and does not absorb the fluids which come in contact with it, and unless compressed there is always a patent channel through which fluid material of any kind can pass. Small holes are pierced in the tubing at short intervals in order to increase its action, and if necessary it can be removed at each dressing, thoroughly cleaned, and then replaced. More than two years ago decalcified drainage tubes, made from the tibiæ and femora of the common fowl, were introduced by Macewen. These fulfil their purpose very well, and taking advantage of the principle of capillarity are threaded with horse hair, which, while maintaining their calibre, forms at the same time an excellent conductor of a serous or sanguineous discharge. The hair is removed at the end of forty-eight hours, but the tube itself needs no further care, and is generally absorbed either whole or in part. A further improvement, which has been carried out in amputations by the same surgeon, is the piercing, by means of a pair of dressing forceps, of the posterior flap in one or more places, and the securing in these of the tubes by means of stitches of catgut. No method can be adopted which more effectually fulfils its purpose than this, and a wound is likely to be most completely drained when several exits are afforded for the escape of discharge. The same principle leads the agriculturist to put in a number of small

drains in preference to one large one, when conducting operations for the removal of superfluous moisture from his fields. (Dr. J. Whitson, Glasgow Med. Journal, Jan. p. 10.)

New Drainage-Tube.—The ordinary india-rubber drainage-tube is often difficult to fix securely, and it is liable to accidentally slip into deep cavities. Occasionally it has been lost in a wound, which has of course remained unhealed until the foreign body has been detected and removed. In empyema the tube is generally introduced two or three inches into the chest, and thus gets blocked up at the bottom of the abscess by the ascent of the diaphragm. At the same time the tube offers very little obstruction to the gradual contraction of the intercostal incision. My drainage-tube is intended by a very simple contrivance to remove many of the disadvantages of the ordinary instrument. It consists of an elastic tube enclosed at one end in an elastic air-pad, to which a fine tube is attached, and by this the pad can be inflated to the required extent. The inflating-tube is then securely closed with a knot or ligature. The open orifice of the drainage-tube projects only just within the cavity to be drained, a matter to many surgeons of great practical importance. It is especially serviceable in cases of empyema treated by free incision, for when the pad is inflated it becomes fixed in position between the ribs. The pad, moreover, forms an elastic wedge which prevents the escape of fluid around the tube, and exerts a comfortable and even pressure in sustaining the open and dilated condition of the incision. The tube must be introduced with the pad in a state of collapse, and after insertion it can be rendered persistently patent and fixed by inflation. Similar elastic pads, containing two tubes, can be conveniently used for the treatment of empyema by irrigation. The drainage-tube is made in several sizes by Messrs. Arnold and Sons, of West Smithfield. (Dr. J. W. Cousins, Lancet, March 10, p. 404.)

Sutures for Wounds.—In bringing the edges of a wound together it is not only requisite to do so accurately, but to avoid tension on the parts. The introduction of button sutures has greatly tended to relieve this, and we have three different kinds to choose from, those of Ogilvie Will, of Lister, and of Macewen. The first two are for wire alone. The one devised by the last-named surgeon is to my mind much the simplest and best, and while easily and cheaply made, is equally well adapted for either wire or catgut. In many situations gut is the most convenient thread to use, for the ends of one side can be left long without inconveniencing the patient, and when secured by a slip-knot, a greater or lesser amount of

pressure, according as circumstances demand, can be applied at any time without disturbing the parts in an injurious degree. Those who have had to deal with wire in this relation know, that though fulfilling its function efficiently, it is not easily untied, and the ends, whether left long or cut short, do not adapt themselves to the parts in their neighbourhood with the pliancy of gut. The button suture should be made use of first, and as the edges of the wound are thus brought into contact, the subsequent passing of the stitches, of which there are two kinds, can not only be accomplished easily but efficiently. (Dr. J. Whitson, Glasgow Med. Journal, Jan. p. 11.)

AFFECTIONS OF THE SKIN, ETC.

Abscesses.—A totally different method of dealing with abscesses is now carried out as compared with the practice of even recent years. Formerly, after incising the cavity and pressing out the contents, the case, with the exception of frequent syringing, was almost entirely left to nature, while recovery in most instances was tedious. At the present time, and with antiseptic precautions, a much more vigorous line of treatment is adopted, and we have no hesitation in removing the pyogenic membrane *en masse*, interference with which older surgeons looked on with disfavour. In order to accomplish this we make use of Volkmann's spoon, and if the whole of the inflammatory products contained in the cavity are scraped out, two healthy surfaces are left apposed to each other, and which speedily becoming covered with granulations soon coalesce. During the process of healing the less the parts are disturbed the better, and syringing the cavity irritates the tissues composing its walls, and leads to increased discharge—while the progress of cicatrization is interfered with, and as a natural consequence recovery, instead of being accelerated, is considerably retarded. The spoons of Volkmann are extremely useful instruments, and will be found invaluable for the removal of diseased granulation tissue, and similar purposes. They are made of different lengths, and the head of the spoon can be procured in either a round or oval form. (Dr. J. Whitson, Ibid, p. 16.)

Abscesses about the Neck.—A thin curved needle, about three inches in length, was threaded with fine silver wire, mounted on a handle, and passed deeply into the swelling from above downwards so as to admit of drainage. The wire was thus got through the abscess, and the ends were tied together outside the skin. Spirit lotion was now applied on lint and changed three times a day. From the first there was a small

quantity of pus on the lint, and this increased every time until in about a week the abscess was entirely drained. A watery discharge now ensued for three days longer, when the edges of the little wounds began to pucker in. The wire was now removed, and a compress applied over the site of the abscess, and between the two openings, which shortly afterwards closed up, leaving two cicatrices, each about the size of the head of a large pin. These cicatrices were at first red, but gradually became white. (Dr. F. J. B. Quinlan, p. 327.)

Boils.—*Boric Acid.*—Dr. Garrigues states that recently he has employed a paste of boric acid in a number of cases of boils on the face and neck, invariably with the most satisfactory results. The combination he uses is the following:—Powdered boric acid, 4 grammes (ʒ i.); vaseline, 20 grammes (ʒ v.); benzoin powder, 0·50 centigrammes (gr. 7½). This ointment is perfectly non-irritating, and can be preserved indefinitely. The boric acid should be incorporated directly with the vaseline, and not first dissolved in glycerine or alcohol, or it will cause irritation. (Dublin Journal, Dec. p. 480.)

Corns and Warts.—At a meeting of the American Dermatological Association, Dr. Jas. C. White stated he had treated fifty cases, without a single failure, by applying twice daily the following combination:—Extract of cannabis Indica, 10 grains, salicylic acid, 30 grains, collodion, 1 ounce. We have treated several cases of corns successfully by means of this application. (Dr. Napier, Glasgow Med. Journal, Dec. p. 478.)

Eczema of the Anus and Genitals. — *Deficient Kidney Action.*—Insufficient kidney action is a common symptom of eczema of the anus and genitals. There is usually a copious deposit of amorphous urates, and frequent and imperative micturition is not uncommon, the repeated calls to pass urine in the night and the itching often reacting upon each other so as to render sleep almost impossible. He recommends pot. acet. ʒ j., tr. nuc. vom. ʒ ij., inf. quassiæ ʒ iv.,—a teaspoonful in water after meals. This has often to be continued during the whole course of treatment. A large amount of oxalate of lime is sometimes found in the urine in eczema; and this oxaluria may be quickly relieved by means of strong nitric acid in doses of two drops after each meal. (Dr. Bulkley, Med. Times and Gazette, Oct. 28, p. 537.)

Enlarged Glands.—*Electricity.*—Most surgeons are familiar with the treatment adopted by Mr. Golding Bird and termed the electrolytic caustic method. In this his object is to remove the gland by permeating it with chloride of zinc. This he effects by passing a zinc electrode into the substance

of the gland, the silver electrode inlaid on the previously blistered surface. A battery with its circuit completed by a copper wire attached to both is thus formed, the exciting fluid for which is the serum of the blood, the chlorides of which are taken up and replaced by the chloride of zinc. This treatment is most effectual, but can only be applied when caseous or other degeneration has actually occurred; therefore any well-conceived line of treatment which anticipates that stage must be welcome to all. I therefore call attention to the following suggestions, which I have found thoroughly satisfactory in practice. Having selected the gland or mass of glands you purpose treating, have the surface well cleaned and wiped over with a solution of salt. Apply the negative pole of a Leclanché battery, having two cells connected, over the most prominent part, and the positive about three inches apart; keep moving the positive reophore in a circle round the negative quite slowly, till the electrical stimulus has been sufficiently applied. Usually five to six minutes is long enough. On the first occasion two cells are enough, as it accustoms the tissues to the action. On the succeeding applications the effect of additional cells may be tried; but should there be the slightest appearance of inflammatory action, as evidenced by a bluish-white tint under the negative reophore, a couple of cells must be at once disconnected, or the application discontinued on that occasion. I have never used more than eighteen cells continued for three minutes, and have found that from eight to twelve cells give the most satisfactory and rapid results. The length of each application varies for different individuals. In some patients three or four minutes twice daily seemed to suffice; in others a longer application only once answered better. Even different glands, or masses of glands in the same individual, progressed more rapidly under varying conditions of length, strength, and frequency of application. (Dr. H. H. MacDonnell, p. 329.)

Erysipelas.—*White Lead Paint.*—A very great effect is produced upon erysipelas and its congeners by covering the surface with white lead paint. This is entirely due to exclusion of air. I believe that the inflamed skin is not in a state to absorb any material applied on its surface; and by the time that it has recovered the lead is separated from the living tissue by a tolerably thick layer of desquamated and dead epidermis. In a number of well-marked cases, you see this disease yield to a mere local application—to simple occlusion of air; yielding, that is to say, not hesitatingly or doubtfully, but at once and unmistakably. I am by no means prepared

to discard the blood-poison etiology of erysipelas; yet why a general systemic disease should be thus cured by local treatment is very curious, and of course many more trials and successes must be realised before so strange a doctrine can be accepted. A lad, aged nineteen, presented himself with erysipelas of the face, in its early stage. The fauces were much inflamed, red, and swollen; the inflammation passing forwards along the nares, and up the lacrymal duct, had spread to the ala nasi, as also to the lower eyelid, which were red, hard, swollen, and still were enlarging. Temperature 102·4°; pulse 120. He was sent to bed, a purge was administered, and his face was painted with white lead paint. This application was renewed twice; no other remedy was used. In six days the lad was well. (Mr. R. Barwell, p. 332.)

Tannin.—Dr. Spender confirms Dr. Braithwaite's good opinion of tannin in erysipelas. He says, "This solution is quite as beneficial when erythema approaches erysipelas in local and general severity. I give the bare outlines of two cases.—1. A lady, a little past middle age, had a sudden attack of erysipelas all over the left thigh and leg, after a trivial injury. The general health was tolerably good. About a dozen 'paintings' with the solution of tannin were sufficient to drive away every trace of the disease, the swollen skin soon presenting a shrivelled look. 2. A maiden lady in middle life, entrusted to my care by Mr. Clouting of Thetford, suffered from erratic erysipelas on the face, after exposure to cold, in October 1881. The tannin solution was very successful, and a recent letter from Mr. Clouting tells me that the lotion has been frequently used during the last twelve-month with the same good result. Tannin completely dissolves in equal parts of water and spirits of wine; and, when applied to the skin with a camel's hair brush, a delightfully cool feeling follows from evaporation. A proper strength is six grains to the drachm of fluid." (Dr. J. K. Spender, p. 336.)

Picric Acid in Erysipelas.—Dr. Flaminio Tassi of Siena has used a saturated solution of picric acid in the treatment of four cases of erysipelas. It was painted on with a brush over the inflamed part. It appears to have a beneficial action, but the number of cases is too small to enable any definite opinion as to its therapeutical value to be formed as yet. (Practitioner, March, p. 213.)

Lymphatic Swellings in the Neck.—*Iodoform.*—In a case of lymphatic swellings in the neck under Prof. Nussbaum, after incision and evacuation of pus, iodoform was freely introduced into the wound with successful results. Its action is described as excellent in such cases, as it has both a chemi-

cal and a mechanical effect, setting up local irritation, and inducing granulation in the callous tissue. In the case of an officer who had a whole chain of glands simultaneously affected, the sprinkling of coarsely powdered iodoform into the wounds, after evacuation of pus, rapidly induced a complete cure. Iodoform should always be employed in coarse powder. (Dr. Isenschmid, Medical Record, Nov. 15, p. 443.)

MEDICINAL OLEATES.—The true oleates, whilst being more efficacious, are of a stable character very different from the oleic solutions in common use; moreover, from containing less oleic acid, they are much less costly, a point not without its practical bearings. (p. 346.)

Oleate of Mercury.—This is the best local stimulant and alterative application of all the mercurials. When applied to the unbroken skin, it produces marked stimulation bordering on congestion. When rubbed in over tumours, indurations, and glandular enlargements, or thickening of the skin, it exerts a most valuable resolvent and alterative action. It has many advantages over the old mercurial ointments: firstly, being a chemical compound, it is more readily absorbed by the skin; secondly, being soluble in fats, it has great penetrating power; thirdly, it does not become rancid; fourthly, it is cleanly and economical. It is a most valuable remedy in syphilis. It is rapidly absorbed, leaving only a reddened surface, and there is no staining of the linen. It is quite capable of producing the constitutional effects, so must be used with caution. It is best to employ an ointment consisting of one part of oleate with three parts of lard, and of this a piece of the size of a small marble may be rubbed into the thighs, the limbs, or trunk. It is also useful in the treatment of indurations occurring after abscesses, in excess or deficiency of pigment, either as a disease or as the result of applications, in indolent papules, in obstinate ulcers, particularly the syphilitic, and in cases of enlarged testicle. In the indolent and chronic stages of psoriasis, when the patches are thickened, harsh, dry, and cracked, the application of the oleate does much good, although it is usually necessary to previously remove the scales by alkaline baths, oils, water-dressing, or wet packing. In all forms of vegetable parasitic disease the oleate lightly smeared over the surface will not only kill the parasite on the surface, but will frequently, by its penetrating and diffusive action, pass into the hair-follicles and sebaceous glands, and destroy any fungus that may have penetrated beneath the skin. In phtheiriasis, the oleate destroys both the parasite and the nits. The oleate of mercury may be employed advantageously

in combination with other oleates. A mixture of ten or twenty grains of oleate of mercury with one drachm of the ointment of oleate of zinc is very effective in chronic acne and eczema, especially in the fissured variety of the latter, which is so common on the palmar and plantar surfaces. In all syphilitic skin-eruptions, and in superficial ulcers, one drachm of the oleate of mercury with three drachms of the oleate of bismuth, or the same quantity of the ointment of the oleate of lead, acts quickly and decidedly. The oleate of mercury, in the proportion of one or two drachms to one ounce of oil of ergot, forms one of the best and most efficacious oily applications for loss of hair. It is especially useful when the scalp is harsh and dry, and the hairs look dull and lack their peculiar lustre. (p. 347.)

Oleate of Zinc.—One part of this, melted with three parts of a fatty vehicle, forms a most useful ointment. The best results, however, have been obtained with oleate of zinc alone, unmixed with any fatty substance. It is a fine pearl-coloured powder, having a soft soapy feel, very much like powdered French chalk. It is a valuable application in all forms of sweating. (p. 348.)

Oleate of Lead.—This ointment is cream-coloured and semi-solid, of the consistence of simple cerate. When applied to the denuded skin, it exerts both a combined sedative and astringent action, and will arrest morbid discharges, protect the surface, and allay irritation. It is more readily absorbed than either Goulard's cerate, or Hebra's litharge ointment, and it is now easily and cheaply prepared. It is of the greatest use in allaying the inflammation and checking the discharge and itching of eczema. It is useful in simple lichen, and in the hard indurated papules in acne of the face and back. It may often be used with much advantage in combination with one or other of the oleates to be presently described. Thus a good combination in many skin diseases is two drachms of the oleate of lead, with one drachm of oleate of bismuth. This has a most beneficial effect in the fissured form of palmar and plantar eczema. When the inflammation and cracking are very deep and severe, and require a marked stimulation, the addition of twenty or thirty drops of the oil of cade, to two drachms of this oleate, will succeed better. In scabies, an excellent preparation is four drachms of the oleate of lead ointment, with half a drachm of sulphur. (Dr. J. D. Shoemaker, p. 348.)

PRURITUS ANI.—Pruritus ani, says the New York Medical Record, often proves a most annoying and obstinate symptom, persistently refusing to yield to our therapeutic endeavours.

It is, therefore, very comforting to be assured that we have, in two well-known drugs, two equally efficient specifics. Thus, Dr. Steele, of Denver, has found quinia sulphate, rubbed up with only sufficient lard to hold it together, a never-failing specific in this affection. He uses it in both pruritus ani and vulvæ. The nearer you get to the full strength of the quinine, the more efficacious it will prove; and some other physician is similarly confident about the local application of Peru balsam. Hence, we are told, there need be no more itching about the anus, and medicine has achieved a new triumph. (British Med. Journal, Feb. 10, p. 245.)

There is no disease more easily cured than pruritus ani, if the treatment be applied to the real seat of disease—the inside of the rectum. I have not seen a case for the last thirty years that was not cured in a week by the application of camphor-ointment to the surface of the rectum inside the anus. It can be applied by the finger, and rubbed round on the inside. A drachm of camphor should be powdered very finely, but not dissolved by too much spirit of wine, and then rubbed up with one ounce of lard. I have never known this to fail of cure. (Dr. Carson, Ibid, March 3, p. 442.)

I have never known the accompanying lotion to fail, if applied frequently with a soft sponge after washing: ℞ Hydrarg. bichlor. gr. i; liq. plumbi ʒ i; acid. hydrocyanic. (Scheele's) ♏ x. Aq. ad ℥i. I rather think it is the mercury which is the efficacious agent. (Mr. Woodhouse, Balham, Ibid, Feb. 17, p. 339.)

Ringworm.—In a large number of cases all the diseased patches were clearly marked by cutting the hair from, and for half an inch, round them; and then all the small and recent places were blistered with glacial acetic acid, containing four grains to the ounce of corrosive sublimate. The large patches, which had probably existed for weeks or for months, were not blistered. The rest of the hair was cut, and kept moderately short during treatment. The heads were then well washed, and the following ointment was ordered to be rubbed into the entire scalp every morning, and into the diseased patches again every evening, while the head was washed twice a week: ℞. Acidi carbolici (Calvert's No. 2) ʒ vj; unguenti hydrarg. nitratis ʒ x; unguenti sulphurin. ℥ij. M. Croton oil was employed in twenty-three of the cases, either to convert small rebellious patches into kerion, or to remove a few remaining stumps. Many of the isolated stumps were loosened by gently pushing the point of a gold pin, dipped in croton oil, into the individual hair-follicles.

Here I may draw attention to this very useful and practical method of removing isolated stumps, which often resist all other treatment for months. The pin should have a fine but somewhat blunted point, and should be carefully pushed a little way into the follicle, by the side of the diseased stump. The point will carry a little croton oil into the follicle, and this will generally set up sufficient pustulation to cause the loosening and expulsion of the diseased stump in a few days. (Dr. Alder Smith, p. 337.)

Scabies, Pediculi Corporis, &c.—*Mercurial Glycerite.*—The absorption by the skin of any medicament incorporated with a fatty substance is very feeble, except for mercurial ointment. According to M. Vigier, any substance incorporated with glycerine, as iodide of potash, chlorhydrate of morphine, &c., is not absorbed. He considers that this property of glycerine is due to its not wetting the skin. Experiments on himself and his pupils have proven that the active substance thus incorporated never produces its constitutional effects. It is for this reason that he recommends glycerine instead of lard in mercurial preparations for scabies, pediculi corporis, &c., as they have an anti-parasitic effect without being absorbed. The following glycerite, notwithstanding the caustic nature of its principal ingredient, may be used without danger: ℞. Hydrarg. bichlorid. ʒ iss; glycerine, ℥ iij. M. (Med. Press and Circular, Feb. 14, p. 149.)

Sponge-Grafting.—Adjust the sponge in successive thin layers over the wound. These layers, not more than an eighth of an inch thick, must be cut in large slices with a perfectly regular surface. The only method I know by which this can be accomplished is by means of a freezing microtome. I happen to possess a large microtome suited for the purpose, which I employ for cutting sections of the entire brain. It holds an entire Turkey sponge; and, when the latter is frozen, the whole mass can be cut into perfectly regular slices, of any desired thinness. Such a layer can be laid with the greatest facility over the wound, so as to fit into all its irregularities. In a few days, the first layer becomes organised. A second can then be placed over this, and so on, a mass of tissue being thus, in course of time, built up. There is no bagging of pus by this method of applying the sponge, and the danger of putrefaction occurring is reduced to a minimum. Another precaution that is necessary is to see that, where the wound is granulating, the edge of the layer of sponge does not come into contact with the pellicle of young epidermis at the side. If so, the epidermis will undermine it, and cause displacement. There ought to be one

interval of about an eighth to a quarter of an inch between the edge of the epidermis and that of the sponge. (Dr. D. J. Hamilton, p. 339.)

I selected a piece of fine Turkey sponge, and treated it by steeping in dilute nitro-hydrochloric acid, then washing with liquor ammoniæ, and finally setting aside in a one-to-twenty solution of carbolic acid. Dr. Graham, of Perth, asked me to see a private case of an ulcer of three months' standing, during which period it had baffled all ordinary methods of treatment, and to decide if the new method might be resorted to. The sore lay over the lower end of the tibia; its size was that of a half-crown; its surface was exsanguine and hard; and its edges were tightly drawn down towards its floor. As a preliminary to further proceedings, caustic was used to the margins, and a strongly stimulating lotion applied twice daily for two days to the surface. The latter proceeding had no appreciable effect. Scratching with the end of a scalpel was now resorted to, until a freely bleeding surface resulted. A bit of sponge was then applied, and kept bound on for three days, when it was found to have united. In three days more, the patient complained of much pain extending up the limb, and the appearances of erysipelas were found spreading from the ulcer upwards. The patient declared himself subject to idiopathic erysipelas. These indications were judged as advising removal of the sponge, and the act produced considerable pain and hemorrhage. A subsidence of the diffused symptoms took place immediately. As regards the ulcer, the changes were specially noteworthy. What had been the type of indolence and obstinacy among such sores was now the picture of healthy action—the surface abundantly vascular and standing well up towards the level of the skin. The simplest dressings were now sufficient to promote repair; and in three weeks from employment of the sponge recovery was complete. (Dr. J. Ferguson, p. 341.)

The sponges used were the finest grained Turkey I could obtain. They were boiled in a weak solution of hydrochloric acid for some hours, and then steeped for half a day in a strongly alkaline solution of creasote. Before application, they were rinsed in hot water, and cut in very thin slices; and the wounds were syringed with the same antiseptic solution in which the sponges had been immersed, in which also were dipped the gutta-percha and lint. A single layer of each material was applied in the following order—sponge, gutta-percha, lint—and the whole was covered with a broad strip of India-rubber plaster, applied so as to secure firm pressure. In conclusion, I may remark that I have observed that,

unless firm pressure over the sponge be used, the granulations will push the sponge away, instead of growing up through its substance. There are two sets of cases in which I have noticed this adhesion of sponge, where no such adhesion has been intended: 1. Where bleeding cavities have been plugged tightly with sponge, as in excision of the eyeball; 2. Where sponge-tents have been used to dilate the cervix uteri, and have been unavoidably left in longer than usual. In both these sets, firm pressure is probably a *sine quâ non* in obtaining adhesion. (Dr. T. Sanctuary, p. 344.)

Our method is to get the finest Turkey sponge, free from grit, &c., and slice it as thin as possible, soak it in acid nitro-hydrochloric oil for two or three weeks, till all the calcareous and silicious matters are dissolved, then, after repeated washings with water, it has a very soft, velvety feel; this, neutralised by washing with liquor ammoniæ and steeping in carbolic acid solution (1 to 20) for twenty-four hours, is ready for use. A healthy granulating surface is required for it; we prefer that of a burn, especially if there have been loss of subcutaneous tissues. First, we gently scratch the granulations till they bleed slightly, then place pieces of this sectioned sponge about the size of a shilling on the bleeding granulations and they soak up blood, which, coagulating in the meshes of the sponge, forms thereby a temporary adhesion. The superficial wound-surface, if less than two inches square, we entirely cover with sponge; if more than about two inches square, we cover it about half irregularly with pieces of that size, and dress it after the Listerian method with oiled silk, six or eight piles of sanitas gauze, gutta percha tissue, and bandage. Sanitas lotion is generally used afterwards at the dressing, it being not so irritating as carbolic acid. The dressings are usually taken down the second day, and the grafts are then found firmly adherent by the coagulum, and comfortable; afterwards dressed every second day; but great discharge requires daily dressing. The sponge, like a catgut ligature, appears to become completely organised. The graft, partially filled with clot, becomes paler in colour, and especially so at its edges, then, more of a jelly-like and homogeneous consistence; and at the margin, it will become lost in granulation-tissue having no line of demarcation; this invasion continues from without inwards until the last little central-island of sponge-texture, as such, disappears. (Dr. P. W. P. Case, p. 344.)

Vaccination and Erysipelas.—I had a case last year which is very important in relation to vaccination, seeing that so much nonsense is written now-a-days on that subject. I

vaccinated the three months' old infant of an intelligent farmer. Everything went on nicely till the sore began to heal. Erysipelas then began in the arm, and extended with great virulence to the whole body. The child was in extreme danger. I ordered the internal use of tinct. ferri per chlort.; and the case did well. This medicine I have been using in erysipelas for many years, and I have hardly ever seen it fail. I could not understand how the erysipelas came, as nothing of the kind had occurred in my previous experience. The operation was performed by very clean ivory points, and the infection, I thought, was particularly good. It puzzled me completely. The father of the child came to me one day, to say that he had found out the secret. His wife had employed a neighbour's little girl for some time as a day-nurse. They had now ascertained that the girl had erysipelas in her arm at the very time I had cut the baby's arm. This, of course, threw a flood of light on the subject. (Dr. Carson, Coleraine, British Med. Journal, March 3, p. 442.)

Venereal and Ordinary Warts.—Prof. Unna recommends for the treatment both of venereal and of ordinary warts the continuous application of unguentum hydrargyri containing 5 per cent. of arsenic. In the case of a young girl upon whose hands were a hundred or more warts, the application for three weeks of a plaster, containing in each 0·2 square metre 10 grammes of arsenic and 5 grammes of mercury, caused entire disappearance of the disease without any irritation of the healthy skin. Cure was effected not by necrotic destruction of the warts, such as occurs in the use of caustics, but by resorption, as in cases of spontaneous cure. (Practitioner, Dec. p. 468.)

AFFECTIONS OF THE EYE AND EAR.

Cataract.—*Congenital Cataract.*—An anæsthetic having been administered, the lids are separated by a stop speculum, and the operator, steadying the globe with a pair of fixation-forceps, introduces a broad needle set at an angle into the anterior chamber, being guided as to the point of entry by the amount of iris which he desires to remove. If there be an exceptional width of clear lenticular margin, the incision may be made within the corneal area to such an extent that a portion of the peripheral circumference of the iris remains intact; but if, on the other hand, the transparent region be more limited, the pupil must extend further inwards, and and this may be accomplished with considerable precision by accurately defining the position of the corneal wound. The broad needle should be introduced at an exact right angle to

the cornea, so that it may enter the chamber without travelling unduly between the corneal layers. As the point of the needle is seen within the chamber, the handle of the instrument should be depressed, so as to avoid the possibility of wounding the lens. The needle should be slowly withdrawn, otherwise a rush of aqueous humour may entangle the iris in the wound. The operator then introduces a small blunt hook, known as Tyrrel's hook, also curved at an angle like the needle. The hook is passed in on the flat beyond the pupillary margin, which is caught by inclining the free edge of the hook slightly downwards. Steady traction should then be made towards the wound; but immediately before emergence the hook must be gently rotated, so that its free edge presents a little forwards, to secure an easy exit, and to prevent its entanglement in the corneal aperture. The portion of iris which has been drawn out, and is still held by the hook, should be cleanly excised with small blunt-pointed curved scissors, close to the cornea, by that much to be desired, but not always available, coadjutor, a competent assistant. If gentle friction be then made with the upper lid over the puncture, the iris floats back into the chamber, and all the elements of a well accomplished iridotomy are secured without its palpable dangers. Nothing further is needed but a light water-dressing and a few days of rest. I may add, that the limitation in the size of the pupil which this method secures, obtains for us a better optical result than can be produced by any iridectomy performed with keratome and forceps. (Mr. Anderson Critchett, p. 353.)

Couching.—In a limited number of patients with senile cataract, such as those of great age with severe cough, or a marasmic constitution; or where suppuration or hemorrhage has followed extraction in one eye; or where previous examination of the patient before the lens has become opaque, has shown very fluid vitreous humour, and some others, recourse may be had to this operation. (Dr. E. Andrew, p. 361.)

Extraction of Senile Cataract in its Capsule.—At the Shrewsbury Eye Hospital, for a considerable time past no speculum has been used, but a carbolised ligature has been introduced through the skin of the upper lid, which, without inflicting any appreciable injury, forms a most perfect speculum (if I may so call it) for the use either of the operator or of his assistant; it is entirely out of the way, it causes no pressure, and it allows the eye to be instantly opened or shut as frequently as may be desired. The same may be used on both lids, but for the lower it is rarely required. *Section of Cornea.*—This has commonly been made in or near the corneo-

sclerotic junction, the extent of the incision generally varying from five-twelfths to one-third of the circumference of the cornea, according to the condition and size of the lens; when hard, a large corneal opening being required, and *vice versâ*. If in doubt, the larger section has been always made. All these corneal sections have been made with a modified Gräfe's or Beer's knife, even on the temporal side, in preference to Macnamara's triangular knife; this being accomplished by using a double forceps rotating the ball, and thus allowing the knife to be used. When, however, with a widely dilated pupil, the temporal section has been determined on, the triangular knife has been used so as to prevent the almost certain injury by the ordinary knife. The upper section is generally selected if the eyeball be in good condition, if there be no intention of using the scoop, or if an iridectomy is to be performed. The lower section is made, if the eyeball be diseased, especially if the tension be lowered; the temporal or the outer and lower section if the scoop be used, as in either of these positions the instrument can be most easily passed under the lens without rupturing the vitreous body to any great extent. The corneal section of Liebreich and Lebrun also readily allows the removal of the lens in its capsule, with probably less tendency to escape of vitreous humour, but with the disadvantage common to all such openings, the great liability to anterior synechia. A nasal section has been made with a bent triangular knife, but it possesses no advantages. *Iridectomy*, which diminishes the conservative aim of this operation, is only practised when there is considerable tension, posterior synechia, a very shallow anterior chamber, or when the pupil is not dilatable by atropine, or only slightly so. In the first four of these conditions, a moderate or a large portion of the iris is removed. When slightly dilatable, a minute portion is taken away, or a simple incision is made through the iris, by a capsular or other scissors. Under these circumstances, iridectomy is generally performed at the time of extraction; but when the eye is much diseased, it should be done a few weeks previously. *Extraction.*—To render the use of the scoop the exception rather than the rule, the suspensory ligament of the lens has been detached by a new method. With a piece of stiff wire, about one inch in length, mounted on a handle, having the point smooth and rounded, and a line to half a line of its extremity bent at a right angle, an instrument has been formed, to be used in the following manner, either before or after the section. If before, a minute opening is made in the corner close to its margin by a small bent broad needle; the instrument is passed into the anterior chamber sideways over the

lens and under the iris until it reaches its distal margin; the point is now turned downwards, the ligament pierced with ease, and following the edge of the lens as a guide, it may be torn to any extent the operator may desire by rotating the ball. If, after the section, the instrument is introduced through any part of the opening found most convenient, and whichever course is followed, being under the iris, that structure is scarcely touched. The endeavour is to tear the ligament with the slightest injury to the vitreous body; and, to accomplish this in the manner above mentioned, the bent portion is made of different lengths, in order to adapt itself to an arched or a flat cataract. The wire is also fixed in a light handle, such as cedar-wood, so that the slightest resistance at the point is communicated to the fingers. The lens being thus quite freed from its attachments—for the slight adhesion to the hyaloid fossa is of no importance—and the patient perfectly quiescent, the lens, by slight pressure with a tortoiseshell curette, may be generally eased through the corneal opening without the aid of any extracting instrument.—I claim for my operation that it lessens external pressure, and prevents internal bruising; that it requires no specula, but provides an easy and safe mode of opening the lids; that it does not mutilate the iris; that the use of the scoop is the exception; that it seldom, if ever, ruptures the capsule, the preservation of which is the chief safeguard in all such operations; and that this mode of operating gives visual results equal, if not superior to any other mode of extraction, with the impossibility of a secondary cataract. (Dr. Edwyn Andrew, p. 361.)

Extraction of Cataract by a Shallow Lower Flap.—I have always avoided the use of anæsthetics, because the comparative brevity of the operation generally renders their employment unnecessary. As to the operation, no speculum or fixation-forceps is employed. The eyeball is steadied only by the operator's fingers in the following manner. The patient, lying flat on his back, either on the operating table, or on a bed, the surgeon stands behind, and, with the fore and middle fingers of the left or right hand, according as it is the right or the left eye which is to be operated upon, gently draws upwards the upper lid. This being done, the middle finger is allowed to descend somewhat, until by gentle pressure on the inner side of the sclerotic, it steadies the globe. At the same time, an assistant draws away the lower lid, and fixes it against the malar bone. An ordinary Gräfe's knife is now used; sometimes, however, I employ one narrower and shorter, the operator using the left hand for the left eye, and

the right for the right eye if he be ambidextrous, and the puncture made in the sclero-corneal junction just below the centre of the pupil. The knife is entered flat, or nearly so, and the counter-puncture having been completed, it is carried downwards for a little distance, and then the edge of the knife is turned directly forwards, and the section finished. The summit of the flap will thus lie about the middle of, or a trifle below, the centre of the space between the pupillary edge and the lower corneal border. The capsule is freely torn with a bent cystotome, and the lids freed from all moisture by portions of lint. The lens now finds a ready exit by means of pressure on the upper lid by the fingers, and by the same plan on the lower, but, generally speaking, with the vulcanite curette. Any remaining cortex is got rid of by gently rubbing the lid over the cornea, and what is well known as the 'lid manœuvre,' and the operation now finished, over each eye is placed a fold of lint, that over the one which has undergone extraction is moistened in cold water, and desired to be kept so for the next few days, and then a thin bandage completes the dressing. The eye is, usually speaking, left undisturbed until the fourth day. At this time, a peep is made to see that everything is favourable; and, if so, the patient is allowed to sit up, for a little, on the fifth or sixth day. With respect to the performance of iridectomy, the rule I follow is simply this. Should the iris show signs of clinging about the wound after the escape of the lens, and not readily receding, a small piece is cut off; or, indeed, sometimes I excise a portion before the removal of the cataract, or the rupturing of its capsule. If, for various reasons, I fancy there be a little more risk in the operation in a given case, an iridectomy is performed. Generally speaking, it is almost a matter of indifference as to whether a segment of iris be removed or not, as I am not certain that cases do better for the iridectomy. For cosmetic reasons, one would avoid it as much as possible. (Mr. Simeon Snell, p. 367.)

Modifications of Graefe's Linear Operation.—1. I always have the pupil well dilated with atropine on the morning of the operation. 2. I always give an anæsthetic, unless it be contraindicated, or unless I can be sure of the complete quietude of my patient. Only six per cent. have been operated on without. 3. The section is corneal, but sufficiently peripheral to be sub-conjunctival; the puncture and counter-puncture, which are a little lower than in von Gräfe's operation, alone being just outside, or in the sclero-corneal junction, according as there is reason to believe that there is a large or a small nucleus. 4. The section is made with extreme delibera-

tion, with scarcely more pressure than the weight of the knife; and, in making the middle of the section, the edge of the blade is not turned much forwards, the desire being that the middle of the section should be only just within the margin of the cornea. It is important that the knife be sharp, in order that no force be used in making the section, thereby avoiding all risk of disturbing the coats and contents of the eye. 5. Sometimes, but not always, after the section has been made, the end of the section answering to the counter-puncture is just touched by the end of the blade of von Gräfe's knife, so as to make the section of the internal surface of the cornea correspond with the section of the outer surface, as is already the case at the point of puncture. This is important only when the section is somewhat small, or the nucleus large. 6. A period of rest of at least one minute is taken after each step of the operation, to give the eye time to adapt itself to the new conditions of circulation and tension. 7. A free crucial incision is made in the anterior capsule. 8. A gentle coaxing out of the nucleus and all the soft lens matter is made with the smooth back of the curette, taking care that the surface of the cornea is moist before passing the curette over it, avoiding, at the same time, all force, and using the hook at once if the nucleus does not readily escape. (Mr. George Cowell, p. 370.)

Eserine as a Preliminary to Extraction.—If a background of iris is a convenience when operating for the removal of foreign bodies from the surface of the cornea, it is equally advantageous when operating for extraction of cataract; while a deep anterior chamber, and a pupil so contracted as to reduce the chances of subsequent prolapse to a minimum, are so manifestly in favour of the operator, that I cannot doubt that the instillation of eserine will speedily become generally adopted as the first step in the operation of extraction for cataract, as also in some cases where it is necessary to form an artificial pupil, or to remove foreign bodies from the cornea. I generally apply the solution half-an-hour or twenty minutes prior to commencing the operation; which, premising that I always secure complete control of the eyeball by forceps that pierce the sclerotic, may be accomplished, according to the varying exigencies of each individual case, by one or other of the following methods. First, I use an extremely narrow-bladed knife; enter it with the edge directed upwards in the corneo-sclerotic junction on a line with the pupil; bring the knife out exactly opposite, and then cut upwards with a gentle sawing motion until close to the corneo-sclerotic junction of the upper third of the cornea,

when the edge of the blade is turned forwards and the section completed. I then excise a few fibres of the upper segment of the sphincter of the iris, lacerate the capsule, and extract in the usual way. It is only in very exceptional cases that I find it necessary to administer an anæsthetic; and, in a majority of patients, I am in the habit of manipulating the knife so as to get a portion of iris across the edges, completing the iridectomy and section at the same time. (Dr. C. B. Taylor, p. 375.)

Galezowski's Operation for Extraction without Excision of the Iris.—The modification I employ refers to the form of the corneal incision; thus instead of having a large flap whose periphery is at all points 2 mm. distant from the sclerotic border, I make my puncture and counter-puncture in the cornea, just where it joins the sclerotic, but I make the summit of my flap 2 mm. distant from the superior sclerotic border. The incision is therefore entirely corneal. By this procedure the resulting flap is broader and not so high as in the old method of Daviel, and the incision is elliptical instead of being spherical, and in consequence coaptation is easier and cicatrisation more rapid. In giving this form to the incision a gain of 2 mm. on each side is obtained, and this allows even the most voluminous cataract to emerge without difficulty. The pupil dilates under the pressure of the lens and allows its free passage and then returns of itself to its former position, or is replaced by the aid of a fine silver stylet. I have performed fifty-six operations by this method, and have not had any serious subsequent complications. The following are the details of the operation I now almost exclusively perform:—The patient is placed upon an operating table or bed, and his eyelids separated by my articulated spring speculum. I then seize the conjunctiva just below the cornea with a fixation forceps, and gently draw the eye slightly downwards. I then puncture the transparent cornea just at its junction with the sclerotic with a very narrow-bladed von Graefe's knife, and at a point 3mm. above the horizontal diameter. Pushing the point of the knife downwards to the border of the pupil, which has previously been dilated, I press on the capsule, incising it from below upwards, then, slightly withdrawing the knife, I push it into the transverse diameter of the capsule, which I then incise in its entire breadth, after which I make the counter-puncture at the opposite corneal border at a point also 3 mm. above the horizontal diameter. Then inclining the edge of the knife slightly forwards I cut the corneal flap in such a manner that its summit shall be 2 mm. distant from the

superior sclerotic margin. This first step in the operation terminated, I remove the fixation forceps and spring speculum, and let the patient rest for a few seconds. I then continue the operation by elevating thoroughly the upper lid with my little finger, which, as well as the curette, presses upon the sclerotic border of the incision whilst the patient looks strongly downwards. With the thumb of my left hand I gently press upon the inferior border of the cornea, and then the lens is seen first to engage in the iris, then in the incision, and, finally, to emerge from the eye. (Dr. H. Galezowski, p. 372.)

Galezowski's New Operation for Cataract. — With a narrow-bladed knife, such as Galezowski describes, the iris is certain to fold over the edge of the blade as it passes across the anterior chamber, and it is always difficult, and sometimes impossible, to complete the incision without mutilating the pupil. In order to obviate this inconvenience, I have been in the habit of instilling a strong solution of eserine half an hour before commencing the operation, and making the incision with knives such as I have figured. (see *woodcuts*, p. 378.) These instruments possess all the advantages of Prof. Graefe's narrow-bladed knife. Their convex form prevents the premature escape of the aqueous humour, and it is consequently easy to prevent prolapse of the iris, which has always been considered the bugbear of extraction. The bent blade enables the surgeon to use the right hand when operating upon the left eye. The knives have been made for me by Weiss, and are represented the exact size. As I very seldom use an anæsthetic when extracting for cataract, and, during the winter months, usually employ my refracting globe with artificial light, I find no difficulty in deferring the completion of the operation until the pupil is again dilated with atropine, which takes place in about half an hour. (Dr. C. B. Taylor, p. 377.)

Dislocation of the Lens.—Most writers on injuries to the eye, producing dislocation of the lens into the vitreous body, are inclined to take a grave view of the condition; but, from a number of observations made, my opinion is much more favourable; in fact, I have been surprised, under such circumstances, to notice the great power of recovery the eye possesses, and the excellent vision obtained by the aid of glasses, with little or no treatment besides rest. In the majority of such favourable cases, it must be allowed that the lens has been dislocated with its capsule entire, and the external coats of the eyeball left intact. When, from greater violence, other lesions are added, such as rupture of the lens capsule, or rupture of the external coats, with admission of

air containing the dreaded germs of the present day, the prognosis becomes more and more unfavourable in proportion to the addition of these evils; recovery becomes much more slow, as the iris is generally implicated; the vision obtained is less perfect; much more active treatment is required; and, when these evils are conjoined with admission of air, often general inflammation is set up, requiring immediate extirpation of the whole eyeball. It seems, then, to me, that simple dislocation into the vitreous body of the lens encased in its capsule causes little or no irritation, and most commonly has a favourable termination. (Dr. E. Andrew, p. 358.)

EARACHE.—*Sulphate of Atropia.*—Dr. A. D. Williams, in American Chemists' Bulletin, recommends the local application of a solution of sulphate of atropia of a strength varying with the child's age. Under three years, one grain to ℥j, over ten years, four grains to ℥j. It may be used warm, and is to be dropped into the ear and allowed to remain ten to fifteen minutes. In grown persons almost any strength may be used. It is practically a specific in the recurring nocturnal earaches of children, but only a slight palliative in acute inflammation of middle ear, and in acute inflammation of the external meatus. (British Med. Journal, Oct. 28.)

ECTROPION.—*Transplantation of Skin en masse.*—In a case in which I was induced to replace a large piece of skin that I had removed from the upper eyelid, no precautions whatever were taken, and although the flap had been separated from the body for nearly half an hour, and was apparently quite dead, the wound healed like a cut finger. So remarkable a success made a great impression on my mind, and being unaware at the time of what had been done in this direction by Lefort and others, I came to the conclusion that I had stumbled upon a discovery of some importance. In establishing an artificial anchyloblepharon in order to treat ectropion, transplant tissue, or for any other purpose, it is most important to maintain the eyelashes intact, and, in order to do so, to make the raw surfaces well within the edges of the eyelids, and to maintain apposition by sutures deeply placed. In the three cases related no preparation of the skin to be transplanted, such as scraping the under surface as recommended by Lefort and Wolfe, was adopted; the skin was simply replaced just as it was removed, with the fine reticulated vascular under surface untouched. (Dr. C. B. Taylor, p. 379.)

ENTROPION.—*Spasmodic, of Lower Lid.*—This very troublesome affection is not an uncommon occurrence after operations on the eyes, and keeps up a great amount of irritation. I have

found that the ordinary method of treatment, *i.e.*, traction downwards of the lower lid by adhesive plaster is, as a rule, next to useless, for the lacrimation produced by the entropion soon destroys its action. The removal of a piece of skin from the lower lid of course relieves the patient in a day or two; but if the trouble, which is only temporary, can be relieved without an operation, so much the better. The following plan proves successful. Take a piece of fairly thick soft lead wire, as is often used for styles. Mould it as accurately as possible to the face by commencing from the centre of the forehead, bringing it halfway down the middle of the nose, thence at right angles along its side, and just below the lower lid to the prominence of the malar bone; again at right angles, and now parallel to the first part to the front of the temple. Strap the two ends of the wire firmly to the forehead, and apply a bandage over the strapping. A little manipulation of the wire to the extent of lightly indenting the skin just within the lower orbital ridge, will keep the lid well everted. A piece of thin sheet lead, shaped something like the ordinary strabismometer, often acts equally well. (Mr. W. J. Milles, Ophthalmic Hospital Reports, 1882, p. 387.)

Purulent Ophthalmia.—*Boroglyceride.*—The way in which I have employed it in purulent cases has been to evert the lids, and, after cleaning away all pus and secretion with cotton-wool, to brush the mucous membrane over with a 1 in 10 solution of the boroglyceride, taking care to introduce it well under the upper lid. This has been repeated daily, and in the interval the mother has been instructed to bathe and clean the eyes and lids every hour with a 1 in 40 solution. No other treatment was used, and the cases began to improve at once, being usually well in from eight to ten days. Where one eye only was affected I have, after brushing over the lids, applied a piece of lint soaked in 1 in 20, covered with a pad of cotton-wool, and bandaged the eye, with the result of preventing the sound eye from becoming attacked. In all the cases the cornea was unimplicated. Boroglyceride is an antiseptic introduced by Prof. Barff, and has been used considerably in Germany in surgical cases, and also for domestic purposes, the preservation of meats, &c. It is made by the chemical combination of boracic acid and glycerine in certain proportions, whereby a new compound, boroglyceride, is formed. The following are some of the advantages of this antiseptic: it is readily soluble in cold or hot water; it is odourless, tasteless, and unirritating; a 1 in 10 solution dropped into the eye causes scarcely any smarting; and, according to its introducer, it is innocuous. (Mr. G. Harbridge, Lancet, Feb. 17, p. 273.)

Iodoform. — Dr. Dujardin reports the case of a child, eight months old, who had been suffering for five days from purulent ophthalmia. Both corneas were greyish throughout, and on the point of ulceration. The eyelids were constantly closed, and presented a truly enormous erysipelatous swelling; pus escaped in abundance, unmixed with tears. The general condition was bad; face pale; complete loss of sleep and appetite. At first, according to routine practice, a two per cent. solution of nitrate of silver, neutralised with salt-solution, was applied, and the eyes were then washed freely with pure water. The result was not good; the everted eyelids presented whitish eschars, produced by the cauterisation; both corneas showed well-defined opacities, probably caused by the injurious action of the nitrate of silver; the conjunctival suppuration remained quite as abundant as before, and the swelling of the lids underwent no change in spite of the use of iced-water compresses. Under these circumstances a successful issue was almost despaired of, but he resolved to try the insufflation of iodoform so strongly recommended by German oculists, and dusted the powder all over the conjunctival surface of the lids. The following day an improvement was noticeable; the purulent discharge had decreased considerably; and the eyes began to water, which he regarded as a very favourable symptom. He continued the application of iodoform. During this time the little patient took broth and Malaga wine. The purulent secretion then entirely ceased, the tears ran freely, the corneas returned to a better state, becoming more transparent; the superficial opacities, due to the use of the nitrate of silver, gradually disappeared. Eight days from the commencement of the treatment the child was out of danger. He sent him back to his parents at Roubaix, with only a small opacity remaining in each cornea, which will, doubtless, disappear in a short time. (Dr. Dujardin, Practitioner, Dec. p. 446.)

Ulcers of the Cornea.—*The Actual Cautery.* — As to the value of the actual cautery in these cases there can be little doubt. In *hypopyon ulcer*, it yields frequently very capital results. Three such cases are at present under my observation. In each the ulcer, directly after the cauterisation, has assumed a healthy appearance, commenced to heal, and the pus in the anterior chamber has disappeared. In an example also of the uncommon condition, *bullous keratitis*, the use of the actual cautery yielded excellent results. A case of *marginal pustule*, which seemed disposed to perforate, healed quickly after a slight touch with the cautery. The application of the cautery is not so painful as first impressions

would lead one to fancy, and practically little complaint is made of its employment. It does not necessitate the administration of an anæsthetic. The cauterisation has in some instances to be repeated, and I need hardly add, its application is superficial. By some an iron bulb cautery has been used, and, indeed, I have, in some cases, employed one. Platinum presents, I fancy, however, advantages. It does not readily oxidise and discolour, nor do the organic particles tend to corrode it. Messrs. Pickard and Curry, of Great Portland Street, have made for me a little cautery, which answers well, I think, the purpose for which we require it. The bulb is of platinum, and is about two millimètres in diameter. It is readily heated in the flame of a spirit lamp. Of course a cautery could be employed attached to a battery, or with I suppose the thermo-cautery, but for ocular purposes the advantages would seem doubtful. (Mr. Simeon Snell, British Med. Journal, Jan. 6, p. 9.)

MIDWIFERY, ETC.

AMENORRHŒA.—*Permanganate of Potash.*—In the case of country girls who have 'seen nothing' for a month or two after coming to town this treatment has answered admirably. Often enough patients do not consult their doctor until they are 'overdue,' until the time of the expected period has passed by for some days. Even then the prompt administration of the permanganate will often bring on the flow at once, but should it fail to do so the treatment ought to be continued, and the patient will probably menstruate normally at the next monthly time. Generally our efforts are not crowned with success until the medicine has been taken for at least three or four days, but in some instances the permanganate acted with striking rapidity, the menstrual flow making its appearance after only two or three doses had been taken. It is not necessary to discontinue the treatment on the appearance of the menses; in fact we generally tell the patient to continue taking the pills three or four days longer, finding that it facilitates the flow. The permanganate often succeeds well after the failure of other remedies, such as iron, aloes, nux vomica, strychnia, pulsatilla, nitro-glycerine, and hot mustard baths. Sometimes, however, it is necessary to give it for six weeks or even longer before the desired result is obtained. In those cases where the patient has menstruated only once or twice, and has then entirely ceased for some months, our treatment answers well; the menstrual function is re-established, and thenceforth proceeds normally at every successive monthly period. In some cases there was no actual

amenorrhœa, but the flow was scanty, lasting perhaps only a single day, or it may be only a few hours. Here the administration of the permanganate prolonged the flow, and even in some instances when it had ceased brought it on again. In girls of about fifteen or sixteen, who have never menstruated at all, the permanganate, as might be expected, is not so certain in its action; but even here it not infrequently acts promptly, bringing on the flow at once. *Dose.*—Two one-grain permanganate of potash pills four times a day. (Drs. Ringer and Murrell, p. 399.)

Displacements of the Womb.—The uterus may be either retroverted or retroflexed. It may be so retroverted that it is literally upside down, the cervix being above and the fundus below: a case has been recorded in which the fundus protruded at the anus when the patient defecated. Or it may be retroflexed, being so bent that the posterior surface of the body forms an acute angle with that of the cervix. But so long as the uterus is free to move, and its walls of natural thickness, it makes no difference whether it be bent or straight, or, if bent, whether little or much. Although at the posterior surface the bend may form an angle, there is no angle in the canal. When we examine a case of this class we find the uterus displaced in either of the ways just mentioned; but it is quite movable, neither body nor cervix is enlarged, and there is no tenderness. The only symptoms the patient complains of will be a pain in the sacral region, and also more diffused pain, referred to the loins, lower abdomen, and thighs, and described as a dragging, bearing-down pain, a feeling as if something were falling from her. All these uncomfortable sensations disappear when the patient lies down—that is, when the intra-abdominal pressure is taken off; a point upon which I would lay particular stress. In some cases, in which the symptoms are those, and those only, which slight prolapsus produces, and in which the only morbid condition present is the displacement (including under that term not only the alteration in the position and shape of the uterus, but the changes giving rise to it), that mechanical treatment is most brilliantly successful. A pessary which pushes up the uterus relieves the symptoms at once. It matters nothing, in this class of cases, what the effect of the pessary upon the shape of the uterus may be. It may straighten the uterus or leave it bent; but if it pushes the uterus up, and remains in its position without injuring any part by its pressure, it will relieve. In cases in which congestion is the result of the displacement, it can be cured by raising the uterus, so as to relieve the broad ligaments from

pressure. This is most efficiently done by a pessary which anteverts the uterus, or at least keeps it in the axis of the pelvic brim. A Hodge's pessary will often do this, and when it does, it removes the symptoms with striking rapidity and completeness. But the Hodge's pessary is formed of a thin and hard bar. If it fail to antevert the uterus—if, as it is represented in some books as doing, it presses directly on the tender congested uterus,—it will aggravate instead of relieve the symptoms. This is an essential and practical difference between this class of cases and the one last mentioned: in the former, anything that pushes up the uterus will relieve, whether it does so by directly pressing on it or not, and it matters nothing whether that organ is straightened or remains bent. In the latter a thin hard pessary, if it presses directly on the tender uterus, will do harm; to do good it must antevert it; and if a reflexed uterus be anteverted, the flexion will be removed. Hence relief often coincides with the removal of flexion, although that condition has nothing to do with the production of the symptoms. But often it is difficult and sometimes impossible, to get a Hodge's pessary to antevert the uterus, and we find the best fitting instrument that we can adjust only raises the body of the uterus by pressing directly upon it, and consequently causes discomfort. In such cases I have found the best instrument to be a thick indiarubber ring, which, it is true, presses on the uterus, but, being thick and soft, its pressure is bearable, and as it raises the uterus, it relieves, although not so thoroughly as an instrument which keeps the organ anteverted. (Dr. G. Ernest Herman, p. 383.)

Minor Displacements of the Womb.—It is universally admitted that versions, flexions, and descent are not necessarily the cause of any discomfort or disorder, and this is a cardinal fact in this subject. Think of it. Thousands of blooming, happy, fertile women have displacements. To treat a displacement, simply because it exists, is a grave error, and yet not a rare one. Such simple uncomplicated displacement is not disease. It is the condition of equilibrium of that woman's pelvic viscera, and therefore the displacement is a constituent part of her comfort and health. I may confirm what I have said by reminding you that a woman may have her womb not only displaced, but also monstrously misshapen or distorted by a fibroid or fibroids, and yet have not a pain or an ache or any discoverable disorder. In fact, it would be hard to say what shape and what position of the womb are unnatural, not to say morbid; certainly its shape and position have very wide range within the limits of the

natural or not abnormal. There is a vast number of cases of chronic disordered health in women, of most varied kinds, which are associated with displacement and descent, and it is very common in the present day to regard the displacement as an important, or as the chief factor, in these cases, even when there are no local or direct symptoms whatever. Now, here my respect for my professional brethren forbids my speaking dogmatically, yet I do not hesitate to recommend you not to adopt this view. Very long experience of my own and of the practice of others leads me to regard the displacement in such cases as trivial, not demanding treatment. You may have the displacement without the symptoms, and the symptoms without the displacement; and till we have some evidence, better than we have now, that the symptoms depend on the displacement, I advise you to leave the displacement alone. You cannot successfully treat it, and, if you did, your patient is not nearer to restored health. Again, there is a large number of cases of chronic disordered health, of most varied kinds, associated with displacement and descent, and with local or direct symptoms of uterine, or at least of pelvic, disorder. In these it is always important, if you can, to remove the disorder, whatever it may be, and the symptoms too. Lastly, there are many cases where the local symptoms and the local disorder are alone or predominate. The symptoms of displacement and descent cannot be definitely described. They are often classed as the general symptoms of uterine ailment. Sacrache, lower lumbar ache, aching in the hips, or in the groins, or in the thighs — all aggravated by walking, and still more by standing. To these are to be added feelings of bearing-down, bladder trouble, and rectal trouble. All of these, and more, or none at all, may be present. There is no relation between the degree of displacement and the severity of the symptoms. They are, in fact, associated with the displacement, and, it may be, aggravated by it; but I do not think they are ever the result of the displacement, pure and simple. (Dr. J. Matthews Duncan, Med. Times and Gazette, Dec. 9, p. 685.)

Prolapsions of the Womb treated by Operation on the Round Ligaments.—The pubes are shaven on each side, from the spine of the pubes outwards. The pubic spine is felt with the finger, and an incision made upwards and outwards for two or three inches in length, in the direction of the inguinal canal. The greater or less length depends on the amount of fat that covers the abdominal parietes. By subsequent incisions the depth of the wound is increased until the tendon

of the external oblique muscle is exposed. The external abdominal ring will now be seen, or some vertically directed fibres that run across it will act as the guide to it. In the first incision a small artery (the superior external pubic) is cut across, and should be tied. It is the only vessel in danger, and afterwards the operation is bloodless. The vertical fibres that cross the external abdominal ring should be cut in the direction of the wound, when a reddish-white tissue bulges out, in which is the termination of the ligaments. The final attachment of the ligaments here, there, and somewhere else, are anatomical questions that need not trouble us. All we have got to do is to put an aneurismal needle below the mass of fatty tissue, and raise it up, so that it can be caught by the fingers, and not by forceps. By gentle and careful traction the mass of fatty and ligamentous tissue is pulled out, and the thick ligament is exposed. The small nerve that accompanies it should be cut through, as well as any adhesive bands that bind it to neighbouring structures, until the naked ligament is held in the fingers. When this has been accomplished, the uterus will be felt to move by traction on the ligament, and can be lifted up to the abdominal wall. Having freed the opposite ligament to the same extent, and by the same means, the important question arises as to how far we are to pull these structures out. If the case is one of *retroflexion*, the uterus should be put by the finger or uterine sound exactly into position, and the ligaments pulled upon until they have *control over that position*. It is not only unnecessary, but irrational, to cure the abnormal position by another as abnormal only in an opposite direction. The uterus should be placed in position by the sound, and not by traction upon the ligaments, because traction upon the ligaments alone would, in the first instance, tend to produce prolapse, especially if the retroflexion was a profound or inveterate one. If the operation is performed for *prolapse*, the ligaments should be pulled much further out, so that the uterus just touches the rectum, and only rests slightly upon it. If we draw the uterus up further, and if it is very heavy, its whole weight will be upon the round ligaments. These will stretch, and they may even yield altogether, so that no better, or a much worse, result may be obtained than if we pulled the womb a shorter distance, leaving it some support from the deeper pelvic structures. The number of inches of 'slack' pulled out is no guide. The position in which the uterus should be retained is the true guide. *Method of retaining the ligaments in their new position.*—This has given me more trouble than anything else. I have employed wire ligatures, clamps, silk ligatures, and catgut ligatures. I have cut the

'slack' off; I have tied the 'slack' of one ligament across the middle line to the 'slack' of the oposite ligament. I will only give you the method that I believe to be the best, as the details of my experience would only weary you. You will observe in this dissection that the 'slack' of the ligament is continuous at one end with the intrapelvic portion of that structure, and at the outer end with the various bands that spread in various directions, and that are lost in the fascia of the mons veneris and its neighbourhood. I pass a needle, armed with a strong catgut ligature, through the skin at the outer and lower side of the wound, through the outer pillar of the external abdominal ring, through the round ligament just as it emerges from the depth of the wound, through the inner pillar of the external abdominal ring, and through the skin on the oposite side of the wound to that in which the needle entered. The ligature is pulled through, and knotted in the first coil of the usual surgeon's knot. The depth of the wound is then sponged effectively, the sponge suddenly withdrawn, and the ligature immediately drawn tight, so as to bring the parts into apposition without unduly constricting them. Another ligature is put on in the same way, and perhaps a third, and then the rest of the 'slack' is loosely packed in the inner part of the wound, and retained there by catgut ligatures. The object to be attained is the retention of the ligaments in position without any undue constriction. By rough handling, or by the excessive constriction of ligatures, the ligaments are made to slough, and there will consequently be a much greater risk of failure and of wound trouble. (Dr. W. Alexander, p. 392.)

Labour.—*Bromide of Ethyl.*—M. Lebert, who has been employing bromide of ethyl largely in midwifery cases, accords it great value in simple confinements. It diminishes and finally suppresses the pain, without having any hurtful effect upon the mother or upon the child. He states also that under its influence labour is more rapid and surgical interference rendered less necessary. The subsequent recovery he believes to be speedier, and the tendency to flooding much less than when the drug is dispensed with. (Glasgow Med. Journal, March, p. 247.)

Nausea and Vomiting in Uterine Affections.—*Bromides.*—We often find that in women the subjects of uterine affections, nausea or even vomiting persists for months or even years, and, as a general rule, remedies prove of little use until the original affection or its reflex consequence have disappeared. Dr. Chéron, however, has under these circumstances found

great benefit result from the administration of bromides in an effervescing mixture, of which the following is the formula: No. 1. ℞. Potass. bicarb. gr. 30; potass. bromid. gr. 30; aquæ ʒii. M.—No. 2. ℞. Acid. citric. gr. 60; syrup. simp. ʒi; aquæ ʒiv. M. A teaspoonful of No. 1 to be mixed with a tablespoonful of No. 2, and the mixture to be taken during effervescence. The dose may be repeated every hour or half-hour—the quantities stated in the above formula representing the maximum to be taken per diem. In localised pelvi-peritonitis this mixture often arrests the tendency to vomit, even during the acute stage. (Dr. Chéron, Glasgow Med. Journal, March, p. 245.)

Pessaries.—In the present great abundance of contorted bits of wood, and metal, and vulcanite, and what not, called pessaries, my advice to you is Punch's advice to a young man contemplating marriage—Don't! Think twice before beginning the often baneful practice of using any instrument, teaching a woman to depend on what, if not positively useful, is positively injurious, though perhaps not much so. Many a woman has suffered from, and many a woman has died of, a pessary; but most pessaries, as I find them in use, are nearly innocuous for evil or for good. They are always harbourers of dirt, and they always keep the mind watching the part; they are all liable to decay, and require, if long used, to be renewed. They all are undesirable additions to the contents of the pelvic excavation, and, if they are efficient, must, of course, cause more pressure, though perhaps on different parts, than that caused by the organ or organs which they keep in an altered position. Curious things are anteflexion pessaries. In regard to their giving relief I meantime express no opinion, but I do say that if they give relief it is not by undoing the flexion and keeping it undone, keeping the womb straight. I have seen most kinds of anteflexion pessaries as placed by their inventors, and too often replaced and replaced, but I have never seen one materially modify the flexion. I have myself never used one, and have no intention of doing so. There is another bad and too common practice which I must not omit to mention here, that is, what is called straightening or putting up the womb or replacing it time after time by the probe or finger. This has no other effect than to irritate the organ, for the displacement recurs immediately after the probe or finger is removed, as the practice itself shows. You may replace a descended or retroflexed or retroverted uterus, and keep it replaced by a pessary, and you may so relieve or remove pains. It is quite common to find a pessary give relief in

what may be called a flexion, because that feature of the case is most striking, without the pessary changing the flexion. In such cases the pessary may maintain a diminished degree of descent, and may prevent increase of descent on walking and may save a tender part of the uterus from pressure on sitting. There is no difficulty in explaining such cases; but to comprehend the action of the pessary you should think of the case as one of descent—not of flexion; and this is true of almost all—if not all—cases of flexion. As a matter of fact, I find the majority of versions and flexions, as observed in practice and treated by pessaries, have their whole conditions of displacement quite unaltered by the pessary, even while in. One of the best examples of relief by a pessary is observed in the anteversion (by probe) of an engorged retroverted and descended uterus. Here a well-fitted Hodge is comforting and curative, maintaining the anteversion, elevating the uterus or preventing descent on walking or standing, and preventing relapse into retroversion or retroflexion by keeping the posterior laquear of the vagina pressed against the sacrum. Another notable example of relief is seen in descent with tendency to cystocele, when the irritation of the cystocele pushing at the orifice of the vagina is most annoying. In such, a suitably sized Hodge, or india-rubber ring, often, by its anterior limb, just catches the cystocele, and obviates the tendency to protrusion through the os vaginæ. For each case your pessary must be specially adapted—a boat-shaped or a double-curved—and it must fit the patient in size and contour. Nothing can instruct you in this but bedside experience. Occasionally you have to try more pessaries than one to find the most suitable. Sometimes a woman, whose case you expected to relieve by pessary, can bear none of whatever kind. A pessary, if it is to be useful, will give relief at once, and will need very little attention from you. If you are frequently fitting and re-adapting, you are almost surely doing more harm than good. A well-fitted pessary may be worn for months without being attended to. You must take care that the pessary does not cause ulceration and cut the vagina, and you must have a new one placed when the former one gets decayed. You will find it hard to get any good from a pessary unless you have a fair amount of perineum to support it. A pessary will be inefficient if the vagina is not long enough and capacious enough to allow of its action without strong pressure on the vaginal wall. In flexion or version, without descent of the whole organ, you can do no good to the version or flexion by a pessary: you have no basis or fulcrum to work from. (Dr. J. Matthews Duncan, p. 395.)

POST-PARTUM HEMORRHAGE.—*Hypodermic Injections of Ergotinine.*—Ergotinine is the alkaloid of ergot of rye, insoluble in water, soluble in alcohol or chloroform. One pound of powdered ergot yields three grains of ergotinine. It is indicated in *post-partum* hemorrhage due to imperfect contraction of the uterus. The dose for hypodermic injection is five to ten minims of a solution containing one fiftieth of a grain in twenty minims. This may be repeated, if necessary; but more than twenty minims should not be given. This produces strong and permanent contraction of the uterus, acts more quickly than ergotine (which is only an extract of ergot), and does not cause local abscesses or indurations. Ergotinine is to ergotine as morphia to extract of opium. It was discovered and prepared by Tanret of Paris. (Dr. C. Chahbazian, British Med. Journal, Nov. 18, p. 1004.)

Hemorrhages of Parturition — Intravenous Injection as a Substitute for Transfusion.—It is the *dynamic* rather than the nutritive value of transfusion which is serviceable in combating acute anæmia. This is well exemplified by the success which usually attends saline intravenous injections (employed as substitutes for blood transfusion). From a perusal of the literature on the subject, from the experience of Dr. Little, when cholera was rife, that saline alcoholic intravenous injections were well-tolerated and beneficial, and from my personal knowledge of this plan of procedure, I am convinced that for combating the effects of alarming hemorrhage it is an almost perfect substitute for blood transfusion, and, indeed is, in many respects superior to that method, for it is always readily applicable, whilst the latter is not; the saline fluid can be procured *ad libitum*, whilst the supply of blood is always very limited, and the risks special to blood transfusion clearly do not exist. If two drachms of alcohol be added to a pint of water at 100° F., into which a powder of the following composition has been stirred, the obstetrician is at once provided with a means, similar to that adopted in the case quoted, of combating acute anæmia:—Chloride of sodium, 50 grains; chloride of potassium, 3 grs.; sulphate of soda, 2·5 grs.; carbonate of soda, 2·5 grs.; phosphate of soda ($Na_3 PO_4$), 2 grs. Since this syphon has been adopted at the London Hospital, it has there been successfully employed, and the advantages I have claimed for it have been practically demonstrated. It is applicable not only for the intravenous injection of salines, but also of defibrinated blood, if obtainable. (Mr. C. Egerton Jennings, p. 402.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON OUR PRESENT KNOWLEDGE OF FEVER.

By Walter Moxon, M.D., Physician to, and Lecturer on the Principles and Practice of Medicine at, Guy's Hospital.

It will be well to consider in a general way, and once for all, the great fact of contagion which is common to the more dreaded and deadly of the fevers. What do we know about contagion? Well, we seem on the very brink of knowing all about it, and yet we must perhaps on that very account be cautious. It is well to be circumspect on brinks. And the attitude of science is properly one of caution, especially of caution against the very probable. Science used to be always being misled by the probable. Therefore now let us challenge strictly all evidence when a general conclusion is to be drawn. The general conclusion is none other than that all contagious fevers arise through the entrance into the system of what are called "germs," and that these germs are of the nature of living organisms, such as have from the first microscopical ages been known to microscopists as bacteria spirillum, micrococcus, &c. Many an hour have I spent in watching these, but never thought of associating them or any of their kind with fever; yet what I witnessed many years ago might have awoke a quicker perception to the anticipation of recent discoveries. I was watching floscularia, a stationary species of rotifer, resembling a relatively large bell with a Japanese quaintness of design about its figure. These creatures had come in large numbers in the aquarium at my dining-room window. Their textures are entirely transparent, and I was discovering their sense organs and the ganglion that represents their brain, and in particular I was watching the curious disappearance of a pair of bright-red eyes, which these creatures possess whilst they are young and active—eyes which, like some other creatures, they lose when they grow old and acquire established positions. But, to my great sorrow, my floscularias, which had been thriving and multiplying freely, began to die away before I could finish all the observations I had hoped to include in a paper which is in the Linnæan Society Transactions. As the creatures sickened they became turbid instead of clear; and whilst I was endeavouring to follow the nerves from the

ganglia I saw that the obscurity which clouded them was due to the presence of countless bacteria, exactly such as I am now familiar with in Koch's figures. I now know that I was witnessing an epidemic of bacterium fever in a population of floscularias, but I was not seer enough to see what was before me then, as I looked for and thought only of nerves.

In 1873, Oberheimer of Berlin discovered spirillum in the blood of persons suffering from relapsing fever; and the fact that such organisms are in the blood in relapsing fever is beyond all question. This discovery might at first seem only a further extension of the knowledge we have of scabies through the finding of the itch insect. But there is this vital difference, that whilst the itch insect is living his quiet little life as an unwelcome guest, he creates no general disturbance except for some importunate sensations; whereas when spirillum gets into the system there is fever, and indeed intense fever. Evidently, then, the spirillum has a much more intimate and general relation with the system than the itch insect has, and it becomes a most interesting question what those relations really are. The facts are simple enough; indeed, are they not precisely such as science has long been aware of as occurring in the popularly well-known processes of vinous and acetous fermentation? In the fermentation and in the fever have we not definite systems of turbulent changes determined by the presence of living germs? Even before the discovery of the yeast plant fever was seen to resemble fermentation. Well, there we have a familiar and apparently close analogy to incline us to the view that relapsing fever, at least, is a colonisation by foreign living creatures which in some way breed a poison, or which, as some think, fill up and choke the life out of vital parts of the frame. This is a simple and plain view, and is easily accepted and understood. But the truth may not be so simple and plain, and I wish to draw your attention to an alternative view. This alternative view will appear to you if we ask the question, Are these spirilla—which, by the way, much resemble some spermatozoids—spermatic in any sense useful to recognise? Are they the offspring of the human body endowed with powers to disturb the vital processes of other human bodies? In a general way, there is a range wherein life and fever show enough in common to have been more than merely poetically or metaphorically parallel. For fever is like life in having its times of duration limited though uncertain, and in having its stages so that you can anticipate them, and you can recognise whether the fever is a young or an old fever; and, as our very question shows, fevers reproduce their kind. Here, then, are all the characters of a life: its uncertain yet sure limits; its stages of development; its power of repro-

ducing itself. And so a fever might appear to be another life lived in and by its sufferer, and the human fevers would thus become, as it were, episodic lives supplementing human life; so that you have not quite lived until you have had your fevers; though it may be found for you that you have quite lived when in the middle of one of them.

There may be too much of some sorts of life. In strychnia poisoning or in laryngismus stridulus, for instance, the convulsions themselves cause death. But convulsion is itself an act of life, and so you may die by too much life. Life is a paradox, look at it as you will. You know that from the chemist's point of view life ought to settle itself at once by the several elements obtaining their favourite affinities, and being forthwith satisfied, which they never do until life is over. Life is such a paradox. But from my present point of view life itself is, as it were, a protracted infection of the germ by the sperm, and the living body remains during its life germinal to other infections when suitable spermatic elements come in contact. If this view seems to you more fanciful and supported by less analogy than the fermentation theory, I will at once put before you what is a serious difficulty in accepting the fermentation theory of fever. On this theory is it not very difficult to explain how it comes to pass that one attack of a contagious fever protects from the danger of that fever ever after? This, as we shall subsequently see, is not strictly true, but it is generally true. If fever is merely colonisation with germs who feed upon and live in the body, why should they not come and feed and live again? The only explanation I remember to have seen offered is that the unsophisticated human frame is born with a small amount of some very special food of which the fever germs are very fond, and it is supposed that these little organisms are so fastidious, that they will not take any other nourishment, so that when they have consumed the whole supply of this kind of food, they die or depart to other feeding grounds. Now, seeing that according to biological science these germs are only protoplasm, it would be very surprising if it turned out to be true that minute unspecialised organisms such as swarm in every puddle where organic decay is progressing should refuse to accept any nutriment other than some inborn kind of delicacy; and when you reflect that there must be a number of these delicacies, for no fever germs will pasture on the food of others, and in fact the ambrosia of one is caviare to the rest, it puts no little strain on the highest genius for imaginative scientific belief to suppose that we are all sent into the world each to bear a little special supply for the several kinds of fever germs to thrive upon.

On what seemed to you perhaps the more fanciful view we may suppose that the human frame can cast off spermatic elements which when they enter other human frames engage in an activity remotely like that which originates germination, so that for awhile, if I may so speak, the protracted germination which constitutes life is itself reinfected and diverted. We may suppose that this activity constitutes the fever; and that when once the fever is over and its life lived, it does not return. Were this really the case, would you not expect that these episodial lives would be most probably lived during the actively growing germinal years of earlier existence, or, in other words, would not the fevers attack children? Which indeed they do. Again, if you suppose the immunity from subsequent attacks which fever affords to be due to exhaustion of materials suitable to the parasitic germs, how can you explain those rather frequent cases in which typhoid fever or measles repeats its attack upon the convalescent patient, and even recurs a third time after defervescence? For if you suppose the immunity to be due to exhaustion of a material suitable to the parasitic germs, how can you explain the reappearance of the fever, and therefore of the germs with their supplies, just after the cessation of the first typhoid attack has shown that all supplies were exhausted? But if you concede that this proves the immunity in question not to be due to such exhaustion of supply, then how do you at all explain that subsequent immunity which is so marked and so happy a feature in the history of contagious diseases?

Well, then, the recurrence of typhoid fever cannot be a question of food of the germs. And if not, we must suppose that the repetition which makes three typhoids is due to some measure of time in the life of the typhoid germs themselves, so that each brood lives just the time of one attack, and the next attack is due to other broods whose life has a time limit equal to that of the fever. But these germs are minute unspecialised particles, which reach their little perfection with extreme rapidity, and all that is known of their rate of growth is the reverse of conformable to periods of fever measured by weeks.

On the other hand, if we suppose the fever process to be an episodic vital action between the elements of texture of different individuals, there is nothing contradictory in supposing such vital actions to be sometimes lived through a second or even a third time. Not long ago at the Pathological Society, there was under debate a question how we should explain the late manifestations of so-called tertiary syphilis and the syphilisation of the fœtus whose mothers, having passed through the disorder, are not then actively diseased. And I endeavoured to show that these and similar facts are explicable

by assuming that tissue of recent origin, or tissue by chance left unsyphilised during the syphilitic fever, undergoes impregnation with the syphilis germs which chance to come from habituated older parts of the frame into contact with the new tissue, either through such an intercommunication of the several parts of the frame as is supposed in Darwin's theory of paragenesis, or, else, in the more easily understood instance of the syphilitic fœtus, through the blood of the mother. And I pointed out that the observations of Mr. Tomes on the Haversian spaces of bones, proving that fresh texture arises and replaces old texture in the life of adult bones; that these observations make it probable that similar new formation occurs in other textures, giving rise to new-formed tissue, which would be germinal to or capable of vitally receiving any poison which might be transmitted to it from the already syphilised general textures. If this is true, though I now cannot give you all the evidence in favour of such a view, then, in tertiary syphilis we have occurring within the body of one and the same individual an infection of young and virgin tissue by the poisonous offsets of older tissues charged with a cause of infection to which those older tissues are themselves no longer susceptible. Such a fact would form a step towards the admission of a like infection by the tissues of an habituated individual of the tissues of another not yet habituated.

But I have hitherto overlooked a branch of evidence of the existence of which you must be duly apprised. Though I may be allowed to deal judicially with the facts as offered, and facts are offered by the most accredited observers which would go to prove that the germs of a fever to which mice are subject can be grown into a fungus which produces spores, and that the spores of this fungus will reproduce the fever in other mice, whilst the fungus threads fail to do so unless spores be present. Such researches are, of course, very elaborate, and when admiring the thoroughness and continuity of the work, and the skill and foresight displayed in the views of the experimenters, we must recognise also the extreme difficulties in avoiding fallacy, and the corresponding possibility that fallacy was not avoided; for to obtain the germs of fever about the 10,000th or 20,000th of an inch large, and to plant them and grow them to a fungus, you must be able to follow their individuality; nay, you should never, if possible, lose sight of the individuals. It would be well to identify these germs; and, indeed, it would be much more secure if they could be marked—say, for instance, by tying little bits of blue ribbon upon them to distinguish them from the infinitude of vulgar multitudes of other germs everywhere around them. And it might be said that these germs did deserve such a token of distinction on account

of the sober way in which they behave exactly according to what the commanders in the experiment expected of them. Indeed, if you will trail a little poetry amongst bacteria, they are so numerous and light that they will gather around and give body enough to the form of your conception so as to make good your theory and set it going in living shape; and a biological theory will have a very happy life when enclosed in a body of active germinal infective protoplasm. It is, indeed, the most fashionable creation of the period. But one must not speak irreverently of protoplasm, and our subject is all too serious. The question may appear a very narrow one when we ask whether fever germs are independent parasitic organisms on the one hand, or specialised parts belonging to the human species originally taking their life from the human body. But to be stuffed with foreign germs is really so very different a thing from having your texture life set into new vital action by spermatic particles from the texture of another person, that it would be well to know which is the truth, if we can only find it out. As to the coarser view which has been advanced, to the effect that the merely mechanical action of fermentation germs may cause the phenomena and fatality of fever, we are not without an instance to show us what really are the consequences of the presence in immense numbers of microscopic organisms in the human blood. The recent discoveries concerning filaria sanguinis hominis show that these minute worms may exist in myriads and circulate with the blood without producing any fever, and, indeed, without causing any grave symptoms, and this when these worms are so numerous as to be immediately found in every drop of blood examined. Indeed, the history of filaria sanguinis hominis seems almost to give a crucial negative to any theory which would suppose fever to arise from a merely mechanical action of germs—that is, through mechanical obstruction produced by the germs. For when we thus speak of fever germs as perhaps always present in the blood, you must not suppose that one only has to get a microscope and a slide and put a little fever blood under it to find it full of germs. No, try in any of our cases of typhoid in the wards and you will find these germs by no means very easily discovered or obvious things. At the outset of such an inquiry you must take notice that the blood serum is often crowded with minute particles which must not be confounded with bacteria, and which exist often to a large extent in the blood of healthy persons. During last winter clinical session some of my most acute and intelligent friends searched carefully for germs in the blood of several severe typhoid cases. The result was that one bacterium was seen, only one, but I was told it was a very active one. When I say that Mr.

Booth saw it, you will know it was well seen, for we all regard Mr. Booth as one of the very ablest and very best students at Guy's, but perhaps the main fact was that all were quite sure that there was only one bacterium.—*Lancet, Dec.* 9, 1882, *p.* 974.

2.—ON THE GERM ORIGIN OF DISEASE.

By F. J. B. Quinlan, M.D., F.K.Q.C.P.I., Physician to St. Vincent's Hospital, Dublin.

After explaining the details of the action of the yeast plant in fermenting a saccharine solution and converting the sugar into alcohol and carbonic acid, Dr. Quinlan said: "Similarly in the human body, when the germs of measles enter through the respiratory passages, a period of what is called incubation occurs; the original germs are multiplying. At last the spores are thrown out, and an immense effervescence and fever, with peculiar eruption on the skin and respiratory passages ensue, and go on until the fermentative material in the system is exhausted; and the patient is left convalescent, and as a rule protected against the recurrence of this particular disease. In 1868, M. Chauveau showed that the infective matter of cow-pock, small-pox, and other zymotics consisted of a fluid containing vast numbers of minute particles, and that by diluting and diffusing in water until the particles subsided, the infection power of the fluid was gone; but that as long as particles remained they would multiply indefinitely in the human body. In their memoir on contagion, published in the British Medical Journal of the 21st of last January, Dr. Braidwood and Mr. Vacher describe the infective matter of measles as consisting of numbers of sparkling colourless bodies in the breath of patients, like those found in vaccine lymph, but larger. In small-pox minute organisms are found in the fluid of the pustules, and in typhoid fever Dr. Klein has found them surrounding the affected intestinal glands. The starting point of all these investigations was the discovery that the blood of animals, dead from splenic fever, swarms with minute organisms resembling little rods, and therefore termed bacilli, which multiply and throw out spores just like the yeast plant. The bacilli of splenic fever can be cultivated artificially in an organic fluid, and the crop can be continued from fluid to fluid. If after seven or eight such successive crops a rabbit or a guinea-pig be inoculated with the last fluid, it rapidly dies of the original splenic fever. Still more curious, it was microscopically proved that the blood of animals, where the rods only were developed, rapidly lost its infective power; but that once the spores were thrown out, it retained it for years. This at once explained the reason why,

while some stables and sheepfolds were, after an outbreak, fitted for the reception of animals by mere disinfection, others could not be so cleansed, and had to be destroyed."

The lecturer now entered into a description of small-pox, of vaccination, and of re-vaccination, explaining his belief that vaccinia was a natural attenuation of variolous poison occurring by the passage of the disease through the system of the cow; and that it exactly resembled the artificial, and preventive, attenuation by Pasteur of the zymotic poison of splenic fever, and proceeded to say: "In considering this matter we must remember that the hard and fast lines between different diseases, as laid down in text-books, do not exist in Nature, and that types of disease, like domestic animals and plants, are constantly undergoing evolution and modification of type owing to surrounding circumstances and treatment. The most remarkable recent instance of this is the curious experiment of the Fraulein von Chauvin, who by a course of physical treatment converted a number of Mexican water-breathing axolotls with gills and rudimentary lungs into amblyostomas, similar animals living on land and with lungs. Another disease the result of germs is typhoid fever, in which multitudes of infective germs are thrown off in the ejecta of the patient. Should sewage containing these get into the water supply of a district a whole community may be infected, as occurred at Bangor, North Wales, this year; and a similar result on a smaller scale has frequently happened when water so polluted has been made to do the duty of 'the cow with the iron tail.' Typhoid fever is popularly supposed to be due to bad smells and sewer gas, but it is really due to the germs contained in these; and in reference to this there are two noteworthy points. One is, as shown by Carpenter, that if sewage be poured through Italian ryegrass the spongioles destroy all germs, and leave the resulting fluid so far harmless. The other is, that although germs will pass through the finest filters, filtration through spongy iron entirely destroys them. The well water of Antwerp—now rapidly becoming the greatest emporium of Europe—is brackish, and unfit to drink; accordingly the inhabitants have recently got up fine waterworks in which they apply this spongy iron filtration—which is the invention of Professor Gustav Bischof—on a large scale to the water obtained from the river Nette. Dr. Frankland has visited the Antwerp waterworks at Waelheim, about fifteen miles above that city, and reported on the results of his inquiry. He attaches especial value to the fact that spongy iron filtration 'is absolutely fatal to bacteria and their germs,' and he considers it would be 'an invaluable boon to the metropolis if all water supplied from the Thames and Lea were submitted to this treatment in default of a new supply from

unimpeachable sources.' Another remarkable germ product is the malarial fever of the Roman Campagna, of Algeria, and of other places. Noticing that it always ensued on the breaking up of new ground, Klebs and Crudeli examined this soil and found germs, which, entering through the respiratory passages, produced the well-known malarial affection. It is well-known that the daily use of a small quantity of quinine by the dwellers in ague districts will prevent this affection; and further, that when it breaks out larger quantities of this drug will cure it. Quinine is a powerful protoplasmic poison and destroyer of germs, and its curative action in ague becomes at once intelligible. There is excellent reason to believe that rheumatic fever is also a germ disease; but as this is argued from analogies, and not yet practically proved, we must speak with reserve on this point. Sunlight is a powerful destroyer of germs, which are, on the other hand, much favoured by dirt and damp, and that is the reason why no house is wholesome in all its parts unless it face either east or west, so that the sunlight during half the day may penetrate every part of it; likewise why cellars and deep basement storeys are unfit for human habitation. Nothing has as yet been done to attenuate the poisons of measles or scarlatina; but if we can argue from what has been done in similar cases, it is probable that it will ere long be done; and these maladies, which now rank almost as inevitable diseases, will become as little so as small-pox. We are accustomed to them, but the amount of collective injury which they do to the community is immense. When they break out for the first time in isolated communities, and attack young and old simultaneously, they amount to actual plagues. This occurred a few years ago in the Fiji Islands immediately after their annexation and civilisation. Later back it happened in the Faroe Islands, where the whole population were at once laid low, except some very old persons protected by an outbreak which had occurred about sixty years previously. One of the islands escaped in a very curious way—it is the southern member of the group, and is twelve miles from its nearest neighbour. Learning what was going on in their sister islands, the Suderoe men drew up their boats into their island, and, maintaining an armed guard day and night for three months, threatened instant death to any stranger who would dare to land. This rough quarantine proved perfectly effectual, and when the neighbouring epidemic had burned itself out, this sanitary Thermopylæ stood unharmed. This brief review of germ disease would be incomplete without some notice of its latest and most brilliant advance in the discovery of the tubercle bacillus. Up to a recent period it was generally believed in these countries that pulmonary consumption, although of a hereditary type, was not an

infective disease. In Southern Europe the contrary opinion has always prevailed; and even here cases occurring of undoubted communication of the malady between individuals brought in close personal contact, caused many a doubt to the thoughtful physician. The matter was set at rest by the discovery of Villemin that tubercle was inoculable, and this was followed up by Koch, of Berlin, who discovered the tubercle bacillus, cultivated it, and communicated it to animals. This bacillus is a species of alga, and is found swarming in phthisical cavities and in the expectoration of consumptive patients. Externally, it is very similar to other parasites, which have nothing to say to tubercle. Herr Koch pointed out that the tubercle bacillus can be easily distinguished from all others by its behaviour in the presence of certain aniline dyes. As this bacillus is about the third of the size of a blood globule, any reasonably good microscope will detect it; and we have the diagnosis of pulmonary consumption thus reduced to the simplicity and exactness of a chemical test. The matter was now taken up by the German Government, who established the Imperial Health Office, the creation of Prince Bismarck, appointed Koch as its director, and made him a councillor of State; for in Germany medical science is honoured and decorated, instead of being hunted into foreign countries by anti-vivisection acts. Supplied with ample means, Koch pursues his investigations in a spacious building, and provided with all necessary assistants and accessories. The animals experimented upon are all kept in cages, and each cage represents an experiment, and the greatest care is taken to secure proper ventilation and to avoid all cases of disease arising from defective hygiene. There are curious differences in the susceptibility of different animals to tubercular infection. Rabbits and guinea-pigs are most susceptible, and barn-door fowls still more so. It was at first believed that dogs could not be infected; but it has been found that they can, especially if wholly fed on vegetables. As a rule, herbivorous animals are much more susceptible than carnivorous, a fact which explains the well-known potency of a meat and nitrogenous diet in saving consumptive patients. Further, an abundance of pure air constantly changed is a great protective, and air for a long time re-breathed an injury—another striking analogy. Consumptive infection is carried by the breath or expectoration of consumptive patients; and there is no doubt that while many persons are very susceptible to infection, particularly when in a lowered state of health or badly nourished, or when the tendency is hereditary, many others are not susceptible at all. It is also a consoling fact that tubercular bacilli, unless kept outside the body at a temperature of 100 degrees, at once die, differing,

fortunately, in this respect from those of splenic fever, which, as already described, sometimes linger for years where animals are kept.—*Medical Press and Circular, Nov.* 8, 1882, *p.* 393.

3.—ON A NEW FACT IN THE PATHOLOGY OF PARASITIC ORGANISMS.

By the Editor of the Medical Times and Gazette.

There can be no doubt that the most pressing question of pathology at the present moment is the relation of parasites—in the widest meaning of the name—to disease. Whilst crowds of young and ambitious pathologists are vieing with each other in the discovery of organisms in the various specific diseases, the senior and more sober of us are patiently, but not less anxiously, looking for the determination of the causal link that will connect such organisms with pathological processes. In the case of the lowest parasitic organisms—bacilli and micrococci—it must be confessed that the prospect of success in this direction, however enticing it may be, is not very bright at present; and the various attempts that have been made from time to time, in tuberculosis particularly, to patch together a consistent doctrine from scraps of the old and the new pathology, have proved to be premature and disastrous. In these attempts there has probably been shown far too little disposition to take advantage of well-ascertained facts respecting the life-history and mode of pathological action of the larger, animal, parasites. Valuable analogies might be drawn between these and the lowest, vegetable, organisms. We now understand very fairly the ways in which several of the animal parasites produce disease. Some, like the echinococcus, do so by their simple presence in delicate and vital organs, such as the eye and the heart. Others rob the system of valuable nutriment—for example, the tænia. A third set of parasites lead to anæmia by causing constant hemorrhages on a small scale, to wit, the tunnel-worm in the duodenum, and the bilharzia in the kidney. The trichina sets up general irritation by the emigration of its hordes from the intestines to the muscles. A fifth group cause local suppuration and ulceration, and prevent healing by keeping up constant foulness and discharge; the mycetoma, or fungus-foot of India, being the type of such parasites. Thus we approach the microscopic organisms of the inflammatory and septic processes; and respecting these we shall probably be not far from the truth if we conclude that the oxidation which is at the bottom of their life-processes of growth and multiplication, and the consequent development of heat within the human tissues, must be the cause of the pyrexia and visceral degenerations which accompany them, and by

which they prove fatal. The mode of working of the specific organisms (if such there be) of the chronic constitutional diseases (tuberculosis, leprosy, syphilis, etc.) is, however, quite obscure; and it is in their instance that, as we have said, the analogy of the higher, animal, parasites, promises now to supply a valuable link in tracing the relation of organisms to disease.

The filaria disease, which has attracted so much attention during the last few years, has hitherto remained mysterious in the respect which we are now discussing. There could be no reasonable doubt that the *Filaria sanguinis hominis* was associated with certain "lymph-diseases," such as chyluria, lymph-scrotum, and other forms of elephantiasis; but it was equally certain that in the majority of instances of filaria disease no lymph disease was to be found. In the words of Dr. Manson, "There is abundant evidence that *Filaria sanguinis hominis* does not always, or even generally, give rise to disease. As a rule, parasite and host live together for years in perfect harmony." As our readers know, from the numerous articles by Dr. Manson which we have republished from time to time on this subject during the last six years, the *Filaria sanguinis hominis* which infests the blood is the embryo form of an enormously (250 times) larger worm, which inhabits the lymphatic vessels. Inasmuch as the mature female filaria is also a human tenant, it follows that in every instance of filaria in the blood there must be a parent filaria in some corner of the lymphatic system. But, as we have just said, evidence of the presence of the parent in the shape of lymph disease, is the exception and not the rule, whence it follows that it is only in special conditions —in certain subjects, at particular times, or under otherwise peculiar circumstances—that the mature worm demonstrates her presence by giving rise to chyluria or elephantiasis. At this point, then, the inquiry begins:—What are the conditions that determine lymph disease in filaria disease? What is the link between the mature parasite and elephantiasis? How comes it that in but one subject out of many serious disease is the result of the presence of such a tenant?

The answer to this inquiry has now been given by Dr. Manson, to whose indefatigable labours we are already indebted for a great portion of our knowledge of the structure, life-history, and pathological relations of this remarkable parasite. We publish to-day on another page that part of Dr. Manson's last paper on filaria disease, in the China Customs Gazette, which refers to the special point under discussion. It presents an account of one of the most curious processes of disease in the whole range of pathology. The foundation of Dr. Manson's argument is the fact that the mother filaria—residing, let us say,

in the lymphatics of the scrotum—whilst naturally viviparous, occasionally aborts. This remarkable fact appears to have been sufficiently proved by direct observation of the most satisfactory kind. Now, as the embryo filaria born at full time is a long *filarial* or thread-like body, which can easily pass through the smallest vessels, whilst the premature embryo is contained in a chorional envelope of oval or rounded shape, which is much too large to traverse the neighbouring lymphatic glands into which the current at once sweeps it, the result is obvious. A mature brood pours through the lymphatics into the circulation: an immature brood plugs the lymphatics all around the spot where the mother lies. The lymph-current is completely interrupted; distension occurs behind the seat of arrested flow; and lymph-scrotum is produced. In a similar manner elephantiasis of a limb, or chyluria, may be developed. Thus, in a way quite novel to us, the connexion between parasitic disease and a definite anatomical change of a morbid kind has been successfully traced.

The importance of Dr. Manson's observations is not, however, to be measured by the novelty of the facts from the point of view of the zoologist, or even of the morbid anatomist. It is of the greatest interest to us that filaria disease itself, although it has not yet made its appearance to any extent in Europe, should be thoroughly understood in all its bearings. But the student of general pathology will see in Dr. Manson's discovery an importance far transcending this. It would appear as if he had determined an entirely new manner of working of disease. As far as we know, we have never before been able to prove—even if we have ever before suspected—that certain diseases in man may be due, not to the presence of parasites within him, but to *processes of disease or disorder in parasites within him.* We have generally believed that the more healthy and vigorous the guest, the more injurious is his influence on the unwilling host. No doubt this conclusion was right as far as it went; and the result of such healthy action on the part of parasites was the various kinds of injury which, as we have already seen, they inflict upon the organism. But it appears that we must now widen our views of the method of working of parasitic disease; and none of us can say where these extended views are to be limited. When we consider the extraordinary variety of animal and vegetable organisms that infest the body, we can readily believe that the filaria does not stand alone in its remarkable relations to human pathology. Especially seductive is the suggestion that in the discovery that at least *one disease in man is due to the disease of a parasite*, may be found the explanation of the occasional association of specific constitutional diseases with organisms which under ordinary conditions

must come in contact with the tissues, probably even flourish in the tissues, without obvious bad effect. The diseases of vegetable organisms are, doubtless, as real as the diseases of the plants which we see around us; and no one can tell how far such diseases may not have an influence of the gravest kind upon the health of mankind. At any rate, the doctrine of *the disease of a disease* is a new feature in pathology, with which we must in future reckon; and which, by the almost infinite extent of its bearings, has opened up an enormous field for the pursuit of investigation, and, we trust, for the treatment of disease.—*Medical Times and Gazette, Feb.* 17, 1883, *p.* 182.

4.—ON THE RELATION OF PATHOGENIC TO SEPTIC BACTERIA.

By the Editor of the Medical Times and Gazette.

The supplement to the Report of the Local Government Board for 1881, giving the Medical Officer's report, contains a very interesting paper by Dr. Klein on the relation of pathogenic to septic bacteria. It is the first instalment of a research upon which he has been engaged, and is still pursuing, with the view to investigate, first, whether, and how far, the *Bacillus anthracis* undergoes any change, morphologically and physiologically, when cultivated artificially; and, secondly, whether ordinary bacteria of putrefaction and septic fermentations can, by artificial cultivations, be so modified as, when introduced into the body of an animal, to be productive of disease; that is, whether it is possible for an innocuous saprophyte to assume the properties of an obnoxious pathogenic organism. The result of Dr. Klein's research so far certainly appears to strengthen the probability that pathogenic and septic bacteria are either not transmutable the one into the other kind, or are, at any rate, less readily transmuted than has been supposed. The Medical Officer to the Board, Dr. Buchanan, gives a clear and instructive summary of the outcome of Dr. Klein's paper, which fills nearly forty pages of small print.

Cultivating disease-bacilli in organic liquids and in gelatiniform organic substances, prepared in his laboratory, Dr. Klein has found (as Dr. Koch found before him) appearance of changes, both in form and potency, to be undergone by the bacilli. Of some such apparent changes, Dr. Klein shows that they were really due to the presence, in the cultivation material, of a new bacillus accidentally introduced; and he points out that a mere overgrowth of the one bacillus by the other, as corn may be overgrown by weeds, is not to be confounded with a true change in the original organism, or in its own inherent qualities.

The specific bacillus of a disease, as illustrated by the case of anthrax, has truly, in the course of its own proper life, times and occasions of remarkable change in its powers. And these are more particularly two. 1. Dr. Klein finds reason for attaching greater importance than had hitherto been assigned to the occurrence of spore-formation in the bacillus. In inoculation with a material containing anthrax bacilli, it is found that an altogether new virulence is acquired by the material as a consequence of the formation of spores in its bacilli; and Dr. Klein, in demonstrating this fact, shows, by some important new observations, that the formation or non-formation of spores in the bacilli is largely a matter of definable circumstance and condition. His results herein appear to indicate some new departure in the investigation of the whole subject. 2. After several weeks of growth of anthrax bacillus in any given specimen of Dr. Klein's cultivation-material, the anthrax bacillus undergoes degeneration—from exhaustion, it would appear, of the pabulum on which it had lived. The change affects more and more threads of the bacillus, and goes on to affect the appearance of the preparation as well under the naked eye as under the microscope. As this degeneration proceeds, the material ceases to injure with certainty the animals into which it is inoculated. But, if any effect at all be produced by inoculation of the degenerating material, it is just the same injury or the same fatal result as would have been produced on the animal by inoculation of the material before degenerative processes had begun in it. The change is in the number of the active bacilli, not in the potency of the several bacilli.

The foregoing changes are those occurring in the course of one cultivation, and in Dr. Klein's observations they were found to occur in exactly the same manner in each of several cultivations made by inoculating successive samples of his cultivation-material, each from its predecessor. Dr. Klein has grown the bacilli of anthrax through ten, twenty, or thirty cultivations, at temperatures ranging between 22° Cent. and 42° Cent., under conditions especially arranged for securing accuracy and intelligibility; and (similar stages of each cultivation being taken for comparison) each successive cultivation showed him, on the rodent animals that he made the subjects of his experiment, results identical with the cultivation that preceded it. In the course of these successive cultivations, made after the manner which M. Pasteur and others believe to produce attenuation of virus, Dr. Klein has not been able to discover any indication of such a diminution of intensity as shall allow of the material of a late cultivation being introduced into the body of an animal without killing it or doing it

a serious injury, but with the result of protecting it thenceforth against death from anthrax when the poison of the original disease is inoculated into the animal. Thus, Dr. Klein, without throwing doubt on Pasteur's discovery of a material protective against fatal anthrax in the sheep, would guard us against generalising from Pasteur's experience, and against inferring from it that an "attenuated" virus can be had by the recognised method of successive cultivations in organic liquids at 42° Cent. There seems to be something more than this, Dr. Buchanan remarks, wanted for the production of M. Pasteur's anthrax "vaccin," and the conditions for it have not transpired from M. Pasteur's laboratory.—*Medical Times and Gazette, March* 10, 1883, *p.* 269.

5.—ÆTIOLOGY AND PROPHYLAXIS OF TYPHOID FEVER.

By C. A. Cameron, M.D., F.R.C.S.I., Dublin.

It is admitted that typhoid fever is a specific disease. "Today specific disease is," says Dr. Arnould, of Lille, "synonymous with germ disease. Chauffard proclaimed the specific nature of the disease, but held that the virus might be spontaneously developed in the human subject, under certain circumstances. Yet Chauffard did not consider himself a believer in spontaneous generation. He regarded the morbid germ as the virtual representative not of a being but of a mode." The opinions of Chauffard are the most elevated as well as the most precise exposition of the theory of a spontaneous and autochthonous typhoid fever created solely by the human economy, external agents playing no other part than that of imparting to it its vital modality. But this theory, beautiful as it may be, does not clear away the obscurity that surrounds the *modus operandi* of those outer agents. If the organism plays a sovereign part in the breaking out of a disease, towards what surrounding circumstances will the student of prophylaxis turn his attention—towards the air, the soil, the water, the food, or the offensive matters? What, then, becomes of hygiene!

Other medical authorities are to be found in the school of spontaneity, but with a marked distinction. According to these authors the specific agent is generally outside man, but is developed spontaneously in some putrid medium, out of which it comes to infect the system. There is no good reason why this agent should not be called "miasm," or even "germ," provided that such a term does not imply an organised germ—living thing. All those who adopt this conception stop short of spontaneous generation. They are not very explicit as to the nature and properties of this specific agent, but they all agree that, once introduced into the human body, the once infec-

tious agent may become contagious—that is to say, that, generated outside man, it finds in the system the conditions favourable to its multiplication and its reproduction. "Typhoid fever," says Léon Colin, "is generated *spontaneously* by infection, and specifically by contagion. Hence the name of infectio-contagious given to this disease, to express in one word this manner of understanding its general etiology. In this school may be included the following authorities:—

Murchison, the author of the pythogenic theory—very opposite to the true contagious theory—of typhoid fever. Here is its definition—typhoid fever can spring up independently of a former case by the fermentation of fæcal matters, and perhaps by the fermentation of other forms of organic matter.

Léon Colin, one of the men who have most carefully studied typhoid fever from its epidemiological point of view. M. Colin's theory is even more comprehensive than Murchison's, and probably the most comprehensive of all the theories which are founded on spontaneity. The genesis of typhogenic agents can take place, according to him, not only in excremental foci, but in all centres of animal putrefaction, and in putridity itself, without precise designation, which are generated by overcrowding of individuals; but one of these circumstances may act alone, or all of them bear conjointly on the case.

Prof. Jaccoud, who, in the discussion which took place in 1877, at the Academy of Medicine, brought together 106 cases, collected from 1865 to 1875, to militate in favour of the theory of the "fæcal origin" of typhoid fever. True that, later on, after an interview with Pasteur, he modified greatly his opinion as to the classification of those cases, and threw overboard the theory of Murchison, of whom at first he seemed a follower.

It is easy to see that this theory, which at first seems to differ from Chauffard's doctrine, in locating outside the system the medium in which is generated the specific agent—*i.e.*, the putrid foci—has also to admit the power of the system to elaborate this specific agent in cases where the putrid focus is not to be found, or its existence is problematical. Then it is supposed that a circumstance of an undefined character—mental depression, for instance—caused the delay of putrid materials in the digestive canal. This putrid focus, though internal, is virtually foreign to the system, as it was destined to be eliminated. The infectious agent develops itself then as it would have done in a water-closet.

Some savants have refused to recognise bacteria and vibrions as the toxic agents of putrid substances. In 1853, A. Stich proclaimed that any fæcal matter, even proceeding from a sound animal, contains a putrid poison which would poison the animal itself if introduced into its veins. Introduced into the

stomach, it is not poisonous unless derived from an animal of another species, or from a sick animal of the same kind. When introduced into the stomach in small repeated doses this matter produces intestinal lesions similar to those present in typhoid fever. Panum (1874) and Arnold Hiller (1875-76) have, by different means, obtained from putrid liquids and meat poisons which do not contain microscopic organisms. The putrid poison found by Hiller presented this peculiarity—that it reproduced itself in the animal, and that its noxious properties increased from generation to generation in the subjects. At the tenth generation, $\frac{1}{120}$ of a drop of this poison in glycerine was sufficient to kill a rabbit after fifty-two hours. The pathological symptoms were fever, dyspnœa, diarrhœa; the lesions were inflammation of the intestines, of the liver, of the prostate, and of the kidneys. These appearances are yet very distinct from those of typhoid fever, which, besides, belongs exclusively to our race. The operators themselves did not claim to have discovered the typhogenic agent. Yet, admitting that their experiments are correct, it is not totally illogical to admit the genesis of typhoid fever in putrid foci as well as the formation, by purely chemical phenomena, of a poison which several menstrua could carry into the system. Sander admits this theory, and in all cases refuses to admit the parasitic theory in diseases proceeding from putridity. Speaking of typhoid fever he always uses the word *Gift* (poison), and never *Keim* (germ). Unfortunately for this theory, typhoid fever, clinically speaking, does not assume the appearance of poisoning; besides, the ptomaines found in putrid albumen (A. Gautier, Selmi) might give the solution of the experiments of Panum and Hiller. They are not, it is true, micro-organisms, but they give rise to diseases very distinct from infectious diseases. It is now almost impossible to look upon typhoid fever as anything but a parasitic disease, therefore non-spontaneous. For the future the basis of all discussions will probably be a germ, real and invariable, totally foreign to the system, whose introduction and multiplication in our tissues and our blood are the more or less direct causes of clinical disorders, and towards which the system fulfils the office of a nutritive menstruum.

In the field of natural observation the origin of the new dogma is traced to the facts of typhoid contagion, so clearly observed by Gendron, Piedvache, W. Budd, &c. In the domain of experimentation and researches in micro-botany this dogma has been from the first a conclusion deduced from facts obtained from the history of some maladies belonging more essentially to other kinds of animals.

It is evident that the want of susceptibility in animals to contract malarial infections or typhoid fever is a cause of dis-

trust in receiving the results that have been brought forward by operators who think that they have imparted to rabbits the specific agent of malaria, such as Klebs and Tommassi-Crudeli, or the agent of typhoid fever, such as Birch-Hirschfeld, Jules Guérin, and more recent writers. As to the results, apart from the death that occurs and the presence of bacteria in the organs, the diseases generated have very little resemblance to malarial or typhoid fever. So many kinds of parasites have been found in typhoid cases that we doubt the allegation that the true *Bacillus typhosus* has been isolated. With respect to the forms of the parasite, Letzerich has described *micrococci* either isolated, in colonies, or in chains, very dissimilar to those of diphtheria and infectious pneumonia, but which, by cultivation, reach a size twice or three times greater than the *micrococci* of the last-mentioned diseases, and says that the typhoid chains are twice as long and more than twice as large. Klebs, on the other hand, describes the *Bacillus typhosus* as large-sized filaments of 50 micromillimetres in length and 0·2 micromillimetres in breadth, without segments or ramifications.

When the spores make their appearance, the filaments may reach a half-micromillimetre. The spores are ranged in a line, and very close. Before reaching this state the Bacillus typhosus is in the shape of small rods, which may also contain spores. The mutation to the form of filament is prepared by a staff of little rods devoid of spores.

Robert Koch thinks that the bacilli, large and short, of Eberth are the only ones which seem to have a specific connexion with the disease. According to him the elongated bacilli of Klebs belong to the putrid parts, and to the class of innocuous bacteria which are developed in ground prepared by pathogenic bacteria, as the author has observed them in a case of charbon in man.

Seat of the Parasite.—Letzerich states that he has recognised the typhogenic micrococci in blood in an isolated state, then in connective tissue, and in the parenchyma of the liver, in the kidneys, in the walls of the intestines, in the cells of folliculi, in the prostate, in the lungs. Klebs finds his Bacillus typhosus in the Lieberkühn glands, and in the connective tissue that surrounds them. He has found it once in the meshes of the pia mater, in the lungs, and in the kidneys. He thinks that there are bacilli in all Peyer's patches as long as the process is developing itself. Eberth finds those specific corpuscles in the thickness, and not the surface, of the glands and the prostate—Koch in the kidneys, the liver, and the prostate. The pathogenic organisms have been observed by Eberth only in half the cases, and it seems that they are to be found rather during the period of the increase of the disease than at its apogee.

It is unfortunately always on rabbits that experiments are carried on in reference to the pathogenic effects of typhoid microzymes. Brantlecht, with from $\frac{1}{2}$ to $\frac{3}{4}$ cubic centimetres of liquid of cultivation, has produced, by subcutaneous injections on rabbits of a certain age, most of the typhoid symptoms. But, strange to say, the bacilli found during the summer in green pond waters give the same results, and as to bacilli found in typhogenic waters they lose at last their specific properties by cultivation. Prof. Letzerich has cultivated in gelatine micrococci obtained from the evacuations and sputa of typhoid patients, and supposes them to be the microbe of typhoid fever. On introducing them by inoculation into rabbits, he has obtained the same results as Brantlecht.

Klebs gives an account of three sets of experiments conducted by his pupil, Dr. Chomjakoff, in which typhoid bacilli have been injected into the peritoneum of rabbits. The animals exhibited immediately an elevation of temperature which attained its maximum on the third day. They all died on the third or fourth day—in two instances from diarrhœa. The lesions were—redness and tumefaction of Peyer's glands, increase in volume of the spleen, cellular infiltration of interstitial tissue of tumefied patches. The presence of micrococci was doubtful, but the peritonitis was in an inverse ratio of the cultivation. The only conclusion that can be arrived at from those experiments is this—In a certain number of cases of typhoid fever micro-organisms are found in the blood, in the tissues and in cases of organs, which are absent in the normal state.

After all, in a medical point of view, the botanical form and individuality of typhogenic microbes have much less importance than their origin and pathological effects. Those organisms modify their form spontaneously, and according to the medium in which they live (Ray Lankester, A. Giard, Tokker, Naegeli, &c.). Why should they not change their properties in changing their habitat? P. Gravitz thinks he has obtained, by cultivation of ordinary moulds—*Eurotium* and *Penicillium*—products of two distinct orders, morphologically identical, but one of them constituting the pathogenic mushroom, very poisonous, while the others can circulate with impunity through the blood of animals.

Dr. Fessinger has obtained similar results; and from the effects produced on rabbits this author goes so far as to suggest the possibility of typhoid fever being generated by ordinary mould, whose spores would have become noxious by spontaneous cultivation.

Buchner holds that he can bring, by sufficient culture, the charbon bacteria to the hay bacteria, and *vice versa*. A.

Wernich (Berlin) asserts the identity of this saprophyte, which infests the human large intestine, with the typhus desmobacteria met with by Klein, Klebs, and Eberth, in the intestinal glands and the organs attacked in a secondary manner.

Dr. L. Poincaré gives, in the *Annales d'Hygiène Publique* for June, 1882, a report on the epidemic of typhoid fever which raged in Nancy during the months of Dec., 1881, and Jan., 1882. One of the typical features of this epidemic consists in the fact that the subjects who left the centre of infection either in the period of incubation or during the course of the disease, gave a higher percentage of mortality than those who were treated in the town. As far as he could ascertain, the former did not spread the contagion. From the very defective sanitary arrangements of the town, it was impossible to trace home the real cause of the present outbreak. Yet, from the class of people who suffered most, it is to be inferred that the sewers played the most important part in causing this calamity. The public school gave 18 deaths out of 53 patients. A female school gave 7 deaths out of 52 patients. In Nancy 532 patients gave a mortality of 59.

Women, especially of the servant class, suffered far more than men, and this will be explained by giving a cursory sketch of the sewerage arrangements in connexion with the houses. 1. The water-closets empty themselves into the sewers. 2. Almost everywhere the junction of the private sewers with the mains is defective. 3. The sinks and water-closet drains are unprovided with syphons. Hence, far from being a source of security against the spreading of diseases, they are actually a connecting link between the affected houses and those not yet attacked The persons most exposed to the emanations are the servants who are constantly making use of the sinks, and the high temperature of the kitchens creates a draft which brings up the air from the main. Acting on the suggestion made by Dr. Vallin in 1881, on the desirability of finding the contagium vivum of typhoid fever, Dr. Poincaré made active researches in the blood, excrements, and muscles of the diseased. Dr. Vallin pointed out the possibility of this germ being a parasite of the order classified by Mégnin as pseudo-trichinæ. The fact of trichinæ giving rise to symptoms very similar to those of typhoid fever, gave to that theory a certain weight. All the results of observations made in that direction and with that object turned out negative.

Dr. Poincaré thinks that the researches would be greatly simplified by generating the disease in animals. But up to this they have resisted the action of the contagium, the only effect produced being similar to ordinary septic poisoning. Peyer's patches have remained sound, and consequently no true typhoid

symptoms have been produced.—*Dublin Journal of Medical Science, Dec.* 1882, *p.* 489.

6.—ON THE INFLUENCE OF BACILLI IN DISEASE.

By J. Cossar Ewart, M.D., Professor of Natural History in the University of Edinburgh.

About the end of March of this year, a new form of fever made its appearance in Aberdeen. The fever began with the usual symptoms; there was a well marked rigor; then a sensation of coldness for some hours, accompanied with great depression; the pulse was rapid, and the temperature increased in some cases to 105° Fahr. In the worst cases, there was delirium. One of the most characteristic symptoms was an affection of the deep cervical glands near the angle of the jaw; the glands enlarged; there were a feeling of fulness about the throat, congestion of the tonsils, and pain along the course of the lymphatics of the side of the neck affected. In from twenty-four to forty-eight hours, the fever subsided, leaving the patient in a state of great exhaustion. In most cases, there was a relapse, which corresponded exactly with the first attack, with the difference that another set of glands and lymphatics were affected. After this relapse, there was again apparent recovery, and then a second relapse; in some cases, there were as many as six relapses, occurring regularly every second day. In nearly all the cases, recovery was slow, and, in some, abscesses formed near the angle of the jaw and in the region of the joints. In three cases, the disease proved fatal.

When an inquiry was instituted, it was found that over three hundred individuals had suffered from this disease, and that all the sufferers had been using milk from the same dairy. A sample of milk secured for examination when the epidemic was at its height was found to contain numerous micrococci, spores of fungi and spores which resembled those of *Bacillus anthracis* —the organism which is associated with splenic fever. When cultivated, the spores germinated, first into exceedingly delicate bacilli, and then into spore-bearing filaments. On inoculating rats with the milk containing the spores, death followed in from eighteen to twenty-four hours. The tissues of the rats, especially in the region of the neck, were infiltrated with bacilli, which, on cultivation, developed into spore-bearing filaments. Inoculation proved both bacilli and spores to be as virulent as the original spores found in the milk. Confirmatory evidence of the relation of the bacillus to the disease, and of the disease to the bacillus, was obtained by the examination of pus from an abscess over the angle of the jaw of one of the sufferers. This pus contained spores and

bacilli similar to those found in, or developed from, the milk. Rats inoculated with a minimal quantity of the pus suffered and died in the same way as the rats infected with the milk and the milk-cultivations. Further investigations proved that the organisms had been added to the milk along with water. The water used at the dairy previously to the epidemic passed through a large concrete cistern (provided with a rough loose wooden cover) placed in the corner of the large byre immediately over the heads of several cows. The spores reached the byre along with the steamed hay used for food, and from the byre they had easy access into the cistern; how they reached the tank in which the hay was steamed has not yet been discovered.

Experiments after the methods employed by Burdon Sanderson, Pasteur, Greenfield, and Buchner, showed (1) that this bacillus could not be converted into the hay-bacillus (*B. subtilis*); (2) that the cultivations became gradually less active until they were quite innocuous; (3) that when the filaments were kept for a time at a temperature which prevented the appearance of the spores, the virulence became attenuated, and ultimately disappeared. Further experiments may show that the attenuated forms are capable of affording protection from the active forms. The bacillus could be cultivated on the fresh cut surfaces of potatoes and in gelatine—the recent methods described by Koch.—*Brit. Med. Journal*, *Nov.* 4, 1882, *p.* 884.

7.—ON AN OUTBREAK OF TYPHOID FEVER AT NEWTON-HEATH, MANCHESTER.

By Henry Tomkins, M.D., B.Sc., Manchester, Medical Officer to Monsall Fever Hospital.

Towards the latter end of last year it came to the notice of the health authorities of the township of Newton-heath, a populous suburb on the northern side of Manchester, that cases of typhoid fever were occurring in their district much in excess of what was ordinarily to be met with, and as no sufficient cause was discovered for the outbreak, and as there was also some doubt whether all the cases met with were really cases of typhoid fever, I was requested by the authorities to make an investigation thereof, and to discover if possible to what the outbreak was due. My inquiries extended from Nov. 24th to Dec. 12th, up to which time I was able to establish the existence or pre-existence of about sixty cases of undoubted typhoid fever, so that there was no doubt about the presence of what may be called a small epidemic of that disease. The next and more important step was to endeavour to ascertain the origin of this outbreak. Up to the third week in October the district

had been free from typhoid, during the next six or seven weeks sixty cases had arisen. The township of Newton-heath extends some two and a half miles from south-west to north-east, but the bulk of the cases were situate in a comparatively circumscribed area, within a radius of some two hundred and fifty to three hundred yards; the number of cases occurring beyond this in other parts of the township did not exceed what may be ordinarily looked for at this season of the year in all large centres of population, or what were occurring at the same time in other parts of Manchester and Salford. The problem therefore to be solved was, To what influence had those residing in the above area (and which for convenience may be called "the infected area") been subjected to which those distant therefrom had not been so subjected? Attention was first turned to the water-supply, but as this is supplied from the Manchester mains and was at the same time being supplied to other districts from the same main, where no typhoid had attracted attention, this could not be suspected of having played any part in the outbreak. The milk-supply was next considered, and to this special attention was paid, as it had been thought highly probable by some residing within the district that this might be the origin of the whole mischief. The facts ascertained in connexion herewith were as follows. In the thirty-three houses in which typhoid was present the milk had been obtained from twelve different sources, one milkman upon whom suspicion fell having supplied twelve houses entirely and three partially, but some of the most severe cases were in those houses not supplied by him; and, further, among his customers outside the above area typhoid did not prevail. Inquiry also at the farm whence the milk was obtained in no wise served to show that there was any reason to suspect that the milk was contaminated. This therefore afforded no satisfactory explanation, nor gave the desired clue. The sanitary arrangements of the district in question, speaking generally, were good. The houses are comparatively new. The excreta, &c., are disposed of in the manner known in Manchester as the pail system, and, together with the ashes and other refuse, are removed weekly. In almost every instance complete disconnexion between the houses and the sewers had been effected—in fact, the entire sanitary surroundings were much better than in other and older parts of the township where typhoid was not prevalent. From the officials of the local board I elicited the following facts. About the middle of September, owing to some suspected defects in a sewer running down a main street situate in this locality, known as Ten Acres-lane, an inspection was made, and it was discovered that the level of this large sewer, eighteen inches in diameter, was defective and alto-

gether at fault, that it could not empty itself, and was found to be more than half full of stagnant decomposing sewage deposit, that it was unventilated, and was choking up and interfering with the proper flow down the branch sewers from the adjacent streets which ran into it. To remedy this it was opened throughout its whole length, taken up, and relaid at a lower level, with sufficient fall, the final closing not taking place until the first week in November, or about the time when the first cases were brought under the notice of the board, and from which time they continued for the next few weeks to increase rapidly.

From careful inquiry I was able to ascertain with tolerable precision that the first case to come under observation occurred about the second week in October. It is pretty well agreed that the average period of incubation in typhoid fever is about three weeks. Now what do we find here? We have the opening of a defective sewer and the disturbance of a large quantity of stagnant sewage matter, in a previously healthy locality, followed within two or three weeks by a series of cases of typhoid, the earliest of which broke out in the street in which the sewer was laid, some of these being among the most severe cases met with during the outbreak, and amongst which the earliest fatal cases occurred, and during the three or four weeks following the final closure of this sewer fresh cases continued to arise, and not until that period had elapsed was there any marked decrease in the number of cases met with. Another point noted was that a larger number of cases lay to the north-east of this sewer than in any other direction; the smaller side streets running off from the main street (Ten Acres-lane) on either side in a north-easterly direction on the one hand and south-west on the other. It was shown by meteorological observations taken in the township that in the fifty-seven days during which the sewer was open the direction of the wind was, in forty-four of those days, from the west or south-west; this would carry the infected air in that direction where the greater number of cases were found. Not that I consider it necessary to lay much stress upon this point, but we know so little as to the properties or powers of life of the contagium of this disease, that we are not justified in saying that air laden with the poison cannot be carried even greater distances than would appear probable in this case without having its infective properties destroyed.

In the Lancet of February 10th, 1883, notice is made of a report of an outbreak of diphtheria in the Holbeach Rural district, where the possibility of conveyance of infection by wind currents is considered. Other epidemic diseases have long been thought to travel considerable distances by the

medium of wind and air currents. But no doubt most of the patients who suffered during this outbreak, living in the immediate neighbourhood, would have come into close proximity to this open sewer some time during the eight weeks it was under repair, so that the wind theory is not necessary to account for them. In four houses, containing six cases, situate some considerable distance beyond the limits of the "infected area," the patients had all been for a greater or less length of time in the immediate neighbourhood of the open sewer. Two of these had lived in the street in which the sewer was open, two had attended school for several hours daily within the infected area, one had visited at the house of a friend there who lay ill with typhoid, and another had passed daily to and fro by the open sewer.

Taking into consideration the whole of the above evidence, there appears to be every justification for connecting the outbreak with the disturbance of this stagnant, unventilated sewer, at a period of the year too when typhoid is most prone to prevail; and although there is nothing new in this conclusion, other outbreaks of a precisely similar nature having been met with by other observers, it yet serves once more to show the danger to be apprehended, and guarded against, when defective drains, especially if containing stagnant decomposing matter, are to be opened up and disturbed.—*Lancet, March* 3, 1883, *p.* 360.

8.—ON THE TREATMENT OF TYPHOID FEVER.

By Professor Jaccoud, Paris.

For the last sixteen years I have submitted the subjects of typhoid fever to a uniform treatment, the principles and effects of which I have detailed in my publications of 1871 and 1872, and I am now desirous of explaining to the Academy the reasons, the means, and the results of this treatment. Its reasons I have derived from two characteristics of the disease, which are so constant and so independent of the individuality of the patient (except in degree) that they justify this therapeutical heresy which bears the name 'uniform treatment.'

These characteristics are, in the first place, adynamia, resulting, on the one hand, from the typhoid infection itself, and on the other, from the duration and intensity of the febrile consumption; and, in the second place, abnormal calorification, this, independently of the consumption it leads to, being the direct source of dangers of another kind, which menace quite specially the heart and brain. Thence two indications, likewise constant, which I formularised in 1872—(1) spare and sustain *from the commencement* the strength of the patient, foresee-

ing the prolonged aggression to which he will be submitted; (2) subtract a portion of the caloric that has been produced, and restrict its formation. I always fulfil these indications, which are constantly the same, from the time that I am certain of my diagnosis: for I can see no valid reason for waiting in a state of inaction, with all the placidity of a spectator, for accidents which I am certain I shall have to contend with. I obey these fundamental indications, then, not only by a treatment as uniform as their own constancy, but by a treatment commenced at once.

This treatment consists of two parts, the one absolutely constant and invariable, the other eventual. 1. The constant treatment is put into force, I repeat, from the time that the diagnosis is certain. It comprises alimentation by means of broths, wine, and especially milk, given in divided doses, so that the daily quantity may amount to between one and two litres. I have never yet met with a case in which it was not tolerated; and, independently of its alimentary properties, milk here possesses the precious advantage of maintaining diuresis at a rate which is sufficient to prevent accidents that, in the stationary period, too often result from excrementitious retention. The constant treatment also comprises alcohol, of which I give from thirty to eighty grammes per diem, according to the individual conditions of the patient and the daily incidents of the disease. To the alcohol I add three or four grains of the extract of cinchona; and I maintain this treatment unchanged until the definite disappearance of the fever. By this sustained alimentation, and this medication, I fulfil at a useful moment the indication derived from the adynamia. It is moreover to be noted, as I have already stated in 1872, that alcohol not only responds to this indication, but that it also fulfils, at least partially, the indication derived from the febrile process, for it lowers the temperature, and it derives on itself, to the advantage of the patient, a portion of the pyretic combustion—the autophagy is diminished. Still, I do not stop here, and from the onset I fulfil the indication derived from the abnormal calorification by means of cold lotions of aromatic vinegar. I have them applied four times in the twenty-four hours when the evening temperature does not exceed 30° C., and six times when it rises to 39.5°, increasing them to eight when it reaches 40° or more. These lotions constantly give rise to a refrigeration of variable duration, which is a true artificial remission; but, besides this, in a great number of cases they exert an effect on the calorigenous process itself, so that at the end of two or three days the thermic curve descends below its primary level—the lotions now being not only anti-thermic, but truly anti-

febrile. The indication derived from abnormal calorification is thus completely fulfilled, a portion of the caloric being abstracted, and its production restrained.

2. So much for my constant, unchangeable treatment; and I now advert, in a passing manner, to the eventual treatment, when I recognise the fever as possessing a *grave character*. To judge of this I have four signs. First, and before all, the continuity of the fever—that is to say, the amplitude of the morning remissions. I regard as insufficient those remissions which are not at least equal to the diurnal oscillations of the normal temperature—say eight-tenths of a degree centigrade. These insufficient remissions denote a special gravity, and tenacity of the febrile process. 2. An interrupted series of evening temperatures above 40°. 3. Absence of depression of the general level of the curve after three days' employment of the lotion. 4. Feebleness of the heart's action, as estimated by the pulse, and especially by direct examination. This element of judgment is independent of the preceding signs, for, with only its own gravity, and while sensibly equal in its course, the fever may act upon the cardiac tissue in a very different manner according to the individuality of the patient,—so that the condition of the heart becomes thus of itself a sign of the first importance in relation to the urgency of eventual treatment.

Whenever, then, the grave character of the fever is demonstrated to me by one or other of the preceding signs, I intervene more actively, and associate with my fundamental treatment antifebrile medicinal agents. I have attained a method which appears to me to favourably fulfil the two conditions of *obtaining the prudent maximum of antipyretic effect with the possible minimum of dose*. The ordinary agent of my medication is quinine in the form of the bromhydrate, which causes less fatigue to the stomach than the sulphate. Since 1876, I replace it, according to the cases, by salicylic acid, but the method of administration is always the same—I proceed by series of two, rarely of three, days consecutively. When the signs already enumerated oblige me to again intervene, I always leave an interval of at least forty-eight hours between the two series of administrations. For either medicine the dose for the first day is from one gramme and a half to two grammes, which is my maximum; that of the second day is half a gramme less; and when there is a third day, the dose is the same as on the second day, or diminished still further by half a gramme, according to the case. Whatever the daily dose may be, I cause it all to be taken within the space of thirty minutes in the morning between ten and half-past ten if I wish to act on the evening temperature, or in the evening between half-past nine and ten when I wish to modify the morning temperature. I decide between

these two eventualities after an examination of the curve, that is to say, accordingly as the grave character of the case consists especially in the rise of the evening figure, or especially in the slightness of the morning remission. In the exceptional cases in which the temperature presents the *inverse type*, I act exclusively on the morning temperature until the fever resumes its normal course. Administered in this way, the doses indicated suffice to obtain between the morning and evening, or between the evening and the morning, a temporary fall of temperature which ordinarily exceeds a degree centigrade, and frequently attains 2°. Moreover, after one or two of these medicinal series, it is the rule for the curve to descend to a lower level, whatever may be the ulterior duration of the fever. One word on my choice between quinine and salicylic acid. In the same dose, and given according to my method, the acid has an antipyretic action which is sensibly equal to that of quinine, so that there is no motive for preference. Salicylic acid also remedies, to a certain extent, the inconveniences due to excrementitial retention, inasmuch as a notable proportion is eliminated under the form of salicyluric acid. It partakes then, with benzoic acid, of the property of favouring the elimination of azotised products retained in the economy during the malady; but I have no need to seek this special action of the acid, thanks to the introduction of milk into the regimen of my patients, so that I have here no sufficient motive to prefer it. But this medicine possesses especially powerful antiseptic properties, and it is for this reason alone that I prefer it whenever I can use it — that is to say, when my choice is not impeded by what I regard as absolute contra-indications to its employment—alcoholism, violent cerebral symptoms, feebleness of the heart, renal determination, and intensity of thoracic symptoms. When, in spite of all these reserves, I find myself at liberty to do so, I employ the acid in place of quinine; and its antipyretic effects, whether immediate or remote, have always seemed to me similar. As to the inconstant indication derived from broncho-pulmonary congestions and stases, I meet it by the persistent application of dry cups, placed morning and evening, to the numbers of from forty to sixty, on the lower limbs and at the base of the thorax.—*Medical Times and Gazette, Feb.* 17, *p.* 191.

9.—INTRAVENOUS INJECTIONS OF AQUA AMMONIÆ FORTIOR IN A CASE OF SEWAGE-POISONING.

By Dr. J. T. Eskridge (in *Philadelphia Med. Times*).

The patient, who had worked for five years as a sewer-cleaner without ever having experienced any unpleasant effects,

and who was a muscular man thirty-six years old, went into a deep privy after having made the customary test of lowering a candle, which continued to burn brightly. Instantly on reaching the bottle he fell, overcome by the gas, which was believed to be either sulphide of ammonium or of hydrogen. At all events it was not carbonic acid gas, and the instance seems to show the danger of trusting to the light test in determining on the safety of entering such vaults.

As he fell, his body became almost entirely covered by fæcal matter, his mouth pointing upwards, but still so close to the foul matter by which he was surrounded that some of the more liquid contents of the well could easily gain access to his air-passages during laboured efforts at inspiration. He remained in this situation half an hour, when a rope was passed around his body, and he was hoisted from the well in an apparently lifeless condition. His rescuer was also very near losing his life, having just time to throw the rope round his companion, when he himself became insensible.

The patient was taken to the hospital, where he was found to have a high feeble pulse, great cyanosis, and frequent convulsions. Digitalis, brandy, atropia, and morphia were all injected hypodermically without effect, except that after the last two had been injected in the left arm the convulsions became nearly limited to that side, but were still very violent. At the end of one and a half hours he was apparently moribund, the respirations being sixty to the minute, of a puffing and jerking character, with tracheal râles, the pulse 200 and very irregular in volume, the convulsions and cyanosis intense and increasing. It occurred to the attendant that, in view of the state of the capillary circulation, the cardiac frequency might be due to clotting of blood in the heart, and that, if anything could be introduced into the circulation to change the condition of the blood and rouse the man's vital powers for a short time, he, being naturally strong, might be able to throw off the noxious influences of the poison. For this purpose, intravenous injection of ammonia seemed to be indicated. One of the superficial veins at the bend of the arm was exposed, and raised with the forceps, and there was gradually injected into it thirty-five minims of the undiluted stronger water of ammonia. (It was supposed at the time to be the diluted aqua ammoniæ.) The pulse was almost immediately lessened in frequency and increased in volume, but the stimulating effects soon began rapidly to pass away. Ten minutes after the first injection, thirty-five minims more were injected into the vein, and this was repeated every ten minutes, until one hundred and forty minims of the ammonia solution had been introduced into the blood. The character of the pulse was improved by each

injection, the respiratory efforts becoming deeper and less frequent. The convulsive movements had by that time nearly ceased. Following the same indication, the injections were then continued every fifteen minutes, with careful watching of the effect. After the tenth injection the pulse was 128 per minute, and did not again rise. After the twelfth and last he was able to swallow stimulants and food, and in ten hours after his attack was apparently conscious, though he had no subsequent recollection of anything that happened that day. The next day he was walking about, feeling quite well, and had no further symptoms except slight nausea and occasional diarrhœa for a few weeks. There was a small abscess at the seat of the injection.

The author concludes that the action of the ammonia was twofold, preventing the tendency to heart-clot, and stimulating the respiratory centres in the medulla. In a similar case he would dilute the stronger water with two parts of distilled water at a temperature of 110° F.—*London Medical Record*, *Feb.* 15, 1883, *p.* 32.

10.—ON BLUE-GUM (EUCALYPTUS GLOBULUS) STEAM IN THE TREATMENT OF INFECTIOUS DISEASES.

By J. Murray Gibbes, M.B., C.M., &c., New Zealand.

My object in this paper is, first, to bring forward a treatment of *diphtheria* I have found very successful; and, secondly, to suggest that a similar treatment might possibly prove equally successful in other infectious diseases. The treatment is that of keeping the patient in an atmosphere of blue-gum (Eucalyptus globulus) steam. It is an acknowledged fact that in blue-gum we have a most perfect disinfectant; not an artificial one, but one of nature's own; one always at hand, for it will grow in temperate climates. The green leaves hung in a bedroom keep it sweet; leaves placed on a wound, steam inhaled from it, or its infusion drank, or injected into wounds, all answer equally well. Prof. Lister speaks highly of the eucalyptus oil for wounds, and it is also spoken highly of in rheumatism. It has not an unpleasant smell, and is tolerated by nearly all. By infectious diseases, I mean those which are caused by micro-organisms. I think it is an established fact that typhoid fever, measles, small-pox, hooping cough, tuberculosis, scarlatina, &c., are caused by fungoid growths. We know that they are introduced into the body by means of dust floating in the air, by milk, water, and food of all descriptions. After they have obtained entrance into the system, they multiply until they produce their specific effects. They pass out of the body in myriads, by means of the breath, perspiration, and discharges.

Infection means the air being poisoned by these germs, and the incubation of a disease is the time required for their propagation into sufficient numbers to interfere with health, and that time seems to be lengthened or shortened according to the quantity introduced, the peculiarity of the different germs, or the state of the system at the time. That a specific germ can alone produce a specific disease is an established maxim. It having been proved that certain germs produce certain diseases, the question arises, How can they be destroyed? Not only how we can cure the disease, but how can we prevent it spreading? Say, for instance, we have a case of scarlatina in a town which had been free from that disease. How can we prevent its spreading through the town? The answer, of course, would be isolation and disinfectants, and it would be perfectly correct, disinfectants having the power of destroying these germs. It is on this point I ask to draw attention—viz., the best and most effectual method of using disinfectants, so that the germs can be destroyed before they can do mischief. I stated that the breath, perspiration, and discharges coming from a patient suffering from an infectious disease are laden with germs all ready to do mischief. At the present time disinfectant lotions, gargles, sprays, and internal remedies are used, and disinfectants poured on the various discharges; so far, so good, but the breath is laden with disease, and the evaporation of the perspiration also assists in poisoning the atmosphere of the room. Ventilation is then carried out to purify the room, but what becomes of the germs? They are dispersed into the external atmosphere to carry disease to others in all directions, and here we come to the root of the matter. If we wish to stay epidemics from spreading, not only must we disinfect the discharges, but we must destroy every germ which comes from the body, either by the breath or otherwise, and to do this effectually we must keep the patient in a disinfected atmosphere. This must be done in every disease which is caused by micro-organisms, and until it is done we shall never be able to stay epidemics. It is the only true scientific method of dealing with disease. How is this best to be done? The discoveries made during the last few years and months revealed to me the cause of our many failures in curing these diseases, and the unscientific methods generally recommended. We gave remedies without having any idea how or why they cured disease; but now science has made vast strides and a great deal that was before obscure is being made plain; we have arrived at the root of the matter, and although, perhaps, we do not as yet know how they cause disease, yet we have a solid foundation to start from. Prof. Lister revolutionised the treatment of wounds when he brought forward his grand treatment of operating in a disin-

fected atmosphere. "The germs cause disease, therefore keep them away," he said, and the world bowed before his dictum feeling that he had hit the right nail on the head. If this answers in wounds, why should it not answer in infectious diseases?

After perusing the papers read before the International Congress, 1881, I felt convinced that the only efficient treatment of infectious diseases was to keep the patient in a disinfected atmosphere for several days, and I had an opportunity of trying my treatment sooner than I expected, for in October of the same year an epidemic of diphtheria broke out in a township sixteen miles from here (New Plymouth, Taranaki). I had made for some years experiments with Eucalyptus globulus in various diseases with most satisfactory results,—namely, in leucorrhœa and other discharges, gonorrhœa, and so-called laryngeal phthisis, croup, bronchitis, &c. My experience with disinfected steam has only been in the above epidemic, and the results have been most encouraging. Thirty-seven cases in which the treatment was carried out recovered without a bad symptom, such as paralysis, without any medicine except castor-oil, and without stimulants, which disproves the statement that diphtheria requires a large quantity of alcohol. The disinfectant I used was made by pouring boiling water on blue-gum leaves. The patients were kept in the moist atmosphere for some days. I mopped the throat with dilute solution of perchloride of iron and glycerine every eight hours, and then covered the pharynx with powdered sulphur. This I did in most cases, but the others recovered equally well. Two young ladies, aged seventeen and nineteen, coughed up complete casts of the large bronchi. An old lady, after I had mopped her throat once, refused to have it done; she had a very dense patch behind and on the right tonsil, the glands of her neck were very swollen and tender, the neck enlarged, and the breath was most offensive. On the third day half of the membrane had come away in small pieces, like grains of rice, the breath was sweet, and the swelling of the neck had nearly disappeared, and she made a perfect recovery. The epidemic was an unusually severe one, judged by the number of deaths of those who were treated by other means. The last cases which occurred took place in two families closely allied, five children and their nurse being attacked. I attended two of the children (the first and last attacked) and the nurse. They recovered, whilst the other three, who were attended by a colleague, died. Local remedies are very good, but they are only a part of the treatment. We cannot perpetually keep spray applied to the throat, the children moving about from side to side of the bed. We must take the cure to them, and

we can only do this by means of steam. My mode of procedure is very simple. I pour boiling water on blue-gum leaves, in a tub, jug, or chamber, which I place beside the bed, and change it every half hour. If only one child is ill in the room I improvise a tent over the bed, either by means of an open umbrella with a sheet above it, or by placing a sheet over the sides or ends of the bed, and enclose the patient. It is wonderful to see how soon the pain in the throat and the swelling disappear, and the fever also. Pain in the stomach is the first symptom most patients suffering from the malady experience, even before the throat is affected. At first I used a simple fever mixture; but I found it was not needed, as the skin acted more or less according to the amount of steam used. The patients were able to eat bread-and-butter, the throat not being sore because I had not burnt it, as is the barbarous treatment recommended by some. In diphtheria the throat is never very sore unless caustics are applied. In simple cases of it many remedies will answer; but if it once gets to the larynx and below it no remedy can touch it except steam. The laryngoscope showed patches on the vocal cords, and the breathing that it had extended lower; and in some cases where suffocation seemed imminent the distressing symptoms would be suddenly relieved by the membrane being coughed up. One young lady remarked, "The steam saved my life."

Although blue-gum steam has answered so well with me, it is not the remedy alone that I wish to bring before the profession, but the principle of the treatment, feeling sure that when a thorough trial has been given to it no other treatment would be found to give such satisfactory results; and if it answers in diphtheria it would answer equally well in other infectious diseases. In typhoid fever the heat would be lessened, the skin kept moist, and the bowels would not have to do double duty. In pertussis it would allay the irritation of the bronchial mucous membrane in the same way that it does in bronchitis, croup, and asthma. In scarlatina the congestion of the pharynx and the skin would be relieved. In so-called laryngeal phthisis it has given most satisfactory results. In influenza the infusion of Eucalyptus globulus is a very popular remedy, and it is one of the most infectious diseases. If blue-gum steam were adopted as a disinfectant, or any other drug, in the ward of a hospital, I should have a boiler outside, with pipes leading into the ward along the floor, with small holes in them to allow the steam to pass through, and have a stopcock at the commencement to regulate the amount of steam. A pipe could also be placed along the wall, with mouth-pieces attached, for those patients who suffered from throat affections. The boiler could be placed in a sand bath, so as to regulate the heat of the water, or a gas

stove used. The leaves could be placed in a net in the water, and changed as often as required. The advantages of the blue-gum steam treatment are that it can be used by ordinary attendants; in fact, a farmer at Tikorangi treated seven cases and cured them. In one of these cases, he informed me, the membrane returned again and again for three weeks. He trusted entirely to the blue-gum steam. No internal remedies are required.—*Lancet, Feb.* 24, 1883, *p.* 316.

II.—THE THERAPEUTIC VALUE OF SULPHUROUS ACID AND STEEL IN SCARLATINA MALIGNA.

By Keith Norman MacDonald, M.D., Cupar-Fife.

That scarlatina maligna is one of the most formidable diseases with which the medical practitioner comes in contact is an undoubted fact, and nothing but the greatest watchfulness and the most vigorous measures are at all capable of battling with its terrible ravages. Every hour lost in the way of treatment adds tenfold to its danger, and, as unfortunately too often happens in the case of children, all hope of recovery may be abandoned when persistent vomiting continues from the commencement, and the power of swallowing is gone.

Whatever pathological differences may exist between malignant scarlet fever and diphtheria, there can be no doubt of a similarity of action common to both, viz., blood-poisoning in one of its most acute and rapidly fatal forms. It was this apparent similarity of action that induced me first to try the effects of sulphurous acid and steel, as they had previously been found more or less successful in the treatment of diphtheria.

Dr. H. Senator, of Berlin, as an exponent of the German view of the pathology of diphtheria, remarks that "in scarlet fever nothing is more frequent than diphtheritis of the throat, and nothing more rare than croup of the air-passages. And yet there can be no greater resemblance between the two local affections than that between the affections of the throat in scarlet fever and in cynanche. The resemblance, indeed, is so complete, that many observers, regarding the anatomical appearances as the essential part of the disease, consider the diphtheritic form of cynanche to be only a scarlet fever without the exanthem, but in other respects look upon both diseases as identical, which, by the way, they are as little justified in doing as they would be in considering every case of acute nephritis as scarlatina *sine* exanthemata, only because the kidney affection is a regular or frequent attendant upon scarlet fever. The micrococci, likewise, are in the throat affections of scarlatina just as numerous, and just the same in character, as these in

cynanche. But if, notwithstanding this, the air-passages in the latter disease are in a manner so very frequently attacked, and in the former so very rarely, we must certainly admit that these organisms, to be met with in the throat, can neither in the one disease nor in the other be the primary and essential cause. The fact that both diseases dispose to similar disorders of the throat, but only the one to a disorder of the air-passages, must be due to the difference in their natures, to a cause at present unknown to us. With regard to disorder of the air-passages, cynanche comes nearer to measles, which, in contrast to scarlet fever, disposes far more rarely to the throat affections, particularly the diphtheritic ones, but regularly leads to diseases of the air-passages, croupous inflammation being the most frequent form which these assume."

Tanner, on the other hand, while holding somewhat similar views, remarks that "diphtheria and scarlatina sometimes occur as epidemics in the same district, while occasionally they co-exist in one individual. Hence some have thought that diphtheria was only scarlatina without any eruption; and they have pointed to the facts that in modified scarlet fever there is now and then an exudation slightly resembling the diphtheritic membrane, while albuminuria sometimes is present in both diseases. Further investigation shows, however, that these affections are distinct from each other, although there may be some analogy between them."

This is enough to indicate, so far as treatment is concerned, that in the present state of our knowledge almost the same treatment may be applied to both diseases; or, in other words, what may be found successful in the one will probably be found equally successful in the other. The late Dr. James Dewar of Kirkcaldy, in a pamphlet published some years ago, drew the attention of the profession to the therapeutic value of sulphurous acid in diphtheria and scarlet fever, and since then, I believe, its efficacy has been firmly established, though not to the exclusion of other remedies. As sulphurous acid is capable of rapidly destroying all animal and vegetable organisms, and being easy of administration, it must continue to rank as one of our most valuable therapeutic agents.

That scarlatina and diphtheria often co-exist in the same individual I have not the slightest doubt, as I have had abundant opportunities of observing the combined symptoms of both diseases in several instances during the course of the epidemic of scarlet fever which has been rife in this town (Cupar-Fife) for the last four months.

The same poison acts differently on different individuals: in one person it produces blood dyscrasia, while in another it terminates in an efflorescence which creates little constitutional disturbance.

As to treatment, I have come to the conclusion, after a great deal of reflection, that to be successful in most cases of scarlatina maligna it must not only be promptly and vigorously, but also intelligently applied, and that when so carried out the worst cases need not be despaired of. In order, however, to attain this end, it is necessary for the medical practitioner to enter minutely into details in giving his instructions, and to see that they are carried out to the letter as far as practicable, and never to be content with the reply, "Oh, he or she won't take the medicine."

My plan of treatment is as follows:—The moment the throat begins to become affected, I administer to a child of five or six years of age 10 minims of the sulphurous acid with a small quantity of glycerine in water every two hours, and I direct the sulphurous acid spray (strength, ʒ ij.—ʒ iv. to the ounce of water, according to circumstances) to be applied every three hours to the fauces—about twenty squeezes; and when that can't be done, to hold the instrument about six inches from the mouth, and use it for a few minutes at a time. The acid solution must be recently prepared, as when it is kept for some time in water it takes up an atom of oxygen and becomes sulphuric acid. It is of some importance to bear this in mind, as the efficiency of the acid treatment depends entirely upon its composition.

At the same time I administer a mixture containing from 3 to 5 grains of chlorate of potash with 7 to 10 minims of the tinct. ferri perchlor. in glycerine and water, more or less according to age, every four hours. I further direct a strong solution of permanganate of potash (ʒ ij. or more to 6 ounces of water) to be held in readiness for laving the lips and mouth several times in the day to arrest the formation of the dark sordes which collect about these parts, some of which should be swallowed, if possible, each time the lotion is applied, gargling being out of the question in young children.

Sulphur should also be burned in the sick-chamber three times a day at least, by placing flour of sulphur upon red-hot cinders on a shovel, and walking about the room with it, thus diffusing the sulphurous acid vapour through the apartment, until the atmosphere becomes a little unpleasant to breathe.

In cases of an adynamic type, when medicine can't be swallowed, the spray and sulphurous acid vapour must be entirely relied upon. It is only in the very worst cases that I employ all the above remedies; for, as a rule, the sulphurous acid alone is quite sufficient to combat most cases of scarlatina anginosa; but when there are white patches at the back of the throat, I invariably administer steel and chlorate of potash as well, and both patients and their friends have frequently

remarked to me, under such circumstances, that an improvement took place after the first dose of the medicine had been swallowed.

Of course, while vaunting the above remedies so highly, I do not forget that other details have to be attended to, such as the importance of fresh air and its free circulation, when that can be attained, the constant presence of disinfectants in the room, such as carbolic acid, chloride of lime, Condy's fluid, &c., the proper disposal of all discharges, the burning of rags containing sputum, &c.; but among the poorer classes of society, where, as a rule, the worst cases are to be met with, details are seldom carried out, and one can only just do the best he can under the circumstances.

As regards diet, milk, beef-tea, and wine should be frequently administered *when possible*, and sucking ice, which, as a rule, can be well borne. In the majority of these cases, however, little nourishment can be taken until all imminent danger is over.

[As to other remedies recommended by various authors, ammonia is nasty, and cannot be taken well by children; carbolic acid has the same fault, and cannot be applied properly. Gargles are also useless in children, because they seldom reach the diseased surfaces, and warm baths and wet-sheet packing are dangerous, because they are never carried out properly in private practice. The hypodermic injection of pilocarpine is a remedy that may give good results hereafter, but I have had no experience of its use.]—*Edinburgh Medical Journal*, *Jan.* 1883, *p.* 605.

12.—ON THE RESULTS OF THE OPERATIVE TREATMENT OF CANCER OF THE BREAST.

By W. Mitchell Banks, F.R.C.S., Surgeon to the Liverpool Royal Infirmary.

In 1877 I published a little article in the Liverpool Reports, based upon a communication read to a meeting of our branch of the British Medical Association. In this it was asserted that surgeons do not remove cancers of the breast. After an interval of five years I feel obliged to reassert the statement. Surgeons, as a rule, do not remove cancers of the breast. They persuade their patients that they do, and they almost persuade themselves. But there is always that little bit which they leave behind, and which they fondly hope will not grow, because it is such a little bit. Alas! that so little leaven should leaven the whole lump! If one turns to the surgery books of a hundred and fifty or two hundred years ago, the true method of removing a cancerous breast will be found.

The breast was laid hold of with great pincers, and having been cut clean off, the surface was seared with a red-hot cautery. Against a proceeding so shocking to the eye modern taste revolted, and so, for many years, surgeons have been removing a little elliptical bit of skin including the nipple, and have been carefully dissecting out the subjacent mamma. Then the remaining skin, all impregnated with cancer germs, has been carefully laid down again and neatly stitched together, so that everything should heal up quickly. Hence, removal of a cancerous breast after this fashion came to be regarded as a comparatively slight operation. Very few people died as an immediate result of it—very few indeed. Unfortunately, at a little later period, they all died from want of a little more of it; so that, looked at from another point of view, it was the most useless of all operations, inasmuch as it never effected a cure. My present contention, therefore, is for a return to the old plan of sweeping everything away and leaving a great hole, if you like. The operation will no longer be the bit of surgical tailoring that it has been, and many more persons will die from it. But many more also will be spared to live useful lives, and escape the horrors of a return tenfold more distressing than the original evil.

I need hardly descant upon the remarkable revolution that has taken place during the last fifteen years in our views as to the removal of infected glands. When I was a student a proposition to clear out the axilla would have been considered madness. In the present paper a principal object is to advocate the removal of the axillary glands, as well as the breast, in *all* cases, whether we can feel them enlarged or not—in fact, to make a clearing out of the axilla a necessary part of the operation for removal of the breast. I have been quietly practising this for three or four years, having been driven to the conclusion that it was the right thing to do, by discovering that, even in those cases where certain glands could distinctly be felt enlarged, when the axilla was opened small ones were discovered, which, although most palpably affected, were quite incapable of being felt from the outside. From this it follows that the usual fumbling in the axilla which is commonly practised is all nonsense. When the glands are as big as walnuts, any first-year student can tell that they are affected. But what I wish with all earnestness to insist upon is that there is a stage—the earliest stage—when they are certainly infected, although to the touch, through layers of fat and skin, nothing amiss can be felt. When a dresser, at the end of his report upon a case of mammary scirrhus, winds up with the stock phrase, "Glands in the axilla not affected," I immediately stop him with, "How do you know? All that you ought to say is that you can't feel them, which is a totally different matter."

Of course, no sooner does one begin to think about this than it seems a very ordinary matter of course; and so it is. But the remarkable thing is that it has not been practically acted upon; and even now, after we have for a good many years got over our nervousness about clearing out the axilla, this proceeding is, by the great majority of surgeons, reserved for those cases where, as I have just observed, the glands are as big as walnuts. When they cannot be plainly felt, the axilla is left untouched, with the result that the patient goes away, and in a few months comes back with the breast cicatrix quite sound, but the armpit filled with a mass of cancer, which has got such a hold of the vessels and nerves as to render its removal impossible. Every surgeon has seen many such cases, and has felt bitterly the disappointment of having removed and cured the original evil and been baulked by its appearance in the glands. Now, at the time of the breast removal, fifteen minutes more work would very likely have sufficed to prevent this. While the glands are yet small, they and the whole fat and cellular tissue of the axilla can be pulled out with the fingers with the greatest ease; and while I am willing to admit that the opening of the cavity adds not a little to the danger of the original operation, I argue that the increased immediate risk is far more than counterbalanced by the increased protection against return of the disease. I therefore most strongly urge the invariable clearing out of the axillary glands along with the removal of the breast, the one operation being useless without the other. As you cannot tell whether the glands are affected or not till you see them in your hand, let them be *always* removed, and so increase the patient's chances of future immunity.

I need not inflict upon you the steps of an ordinary operation for removing the breast and clearing out the axilla; but when a man has done any job a good many times, he is sure to find out some small but perhaps useful details. In most cases these may not be of any importance;—in some they may just turn the balance and save a weakly patient. May I venture to mention a few which have impressed themselves upon me? First, then, as to Listerian antiseptics. As a rule, I employ them; in hospital always. But in the operation under consideration, they have this one great disadvantage, that the spray seriously cools down the patient, and thus lowers her vitality. If the operation is performed in the old-fashioned and useless way, there is very little shock. If performed in the thorough and sweeping way I am now advocating there is a serious shock, seeing that from thirty to forty minutes is the very shortest time in which it can be satisfactorily done. Now, let any healthy woman get out of a warm bed, strip herself

naked to the waist, and then go and lie down on her kitchen table for forty minutes, and I shall be very much surprised if she has not a cold next day. But in addition to this let a cold spray play upon her, and furthermore let a great mass of heat-conserving skin and fat be carried away from the chest, so that nothing but ribs and muscles intervene between the lung and the air; and still let the cool spray play upon her. Is not this likely to be very depressing? I am convinced that it is so; and if the operator, in place of doing his work rapidly, niggles and fiddles about it, so that the patient is kept for a long time under the combined lowering influences of cold spray, anæsthetic, and loss of blood, then the result is that a distinct catarrhal pneumonia very speedily appears. And this I have seen put the patient's life in imminent jeopardy on several occasions. For this reason, in private, where there is but little fear of septic influences, if the patient be weakly I content myself with simply washing out the wound well with carbolic solution and maintaining a modified antiseptic dressing. In hospital I do not use the spray till the operation is pretty well completed, when I turn it on for a minute or so, and drench all the parts well before the final stitching up is done. Another point is this: having detached the breast, I leave it hanging by the axillary end; and then, before attacking the armpit, I draw together as much of the wound as will possibly come, and cover up the remainder with sponge or gauze. This has two advantages: it keeps the cold from the chest, and it enables the operator, if he finds he cannot thoroughly remove the glands, or sees the patient becoming collapsed, to terminate the operation at once. If the cancerous lump is deep in the breast, and the skin over it is quite movable, then some of the latter at the margin of the gland may be kept to cover in the wound. But if any part of it is involved, then a circle should be drawn around the breast, and it should be cut clean off without the remotest regard to flaps or coverings of any kind. These are of secondary moment altogether. The breast being finished with, the incision should be carried up into the axilla about an inch below the margin of the great pectoral muscle. Then comes a strong temptation to dissect down the lower flap, and lay bare the latissimus dorsi and the subscapular artery. There is very seldom any occasion for this; and when it is done, if suppuration occurs, a fine pocket for pus is left. I have twice seen the pus filter its way right to the middle of the back, and have had to make a counter-opening below the angle of the scapula.

With regard to the lower axillary glands, they are capable of easy removal, and even the highest ones can, as a rule, be readily brought down from the very top of the cavity, and pinched off between the nails of the finger and thumb. I have

never yet found occasion to divide the pectoral muscles, as, even in the three instances mentioned in the printed list, where I was unable to remove the glands, I saw them quite clearly, but was afraid to take away so much of the vein, to which they were closely adherent, as would have been necessary thoroughly to remove them. By the way, it is a question if removing a piece of the vein is after all such a dangerous thing as we think. A short time ago, I removed about an inch and a half of the internal jugular vein along with some cancerous glands of the neck, and the patient made a more than usually rapid recovery without the slightest bad symptom. — *Liverpool Medico-Chirurgical Journal, Jan.* 1883, *p.* 91.

DISEASES OF THE NERVOUS SYSTEM.

13.—ON THE THERAPEUTIC EFFECTS OF HYOSCYAMINE IN MANIA.

By Thomas Browne, M.D., Staff-Surgeon R.N., Royal Naval Hospital, Great Yarmouth.

Hyoscyamine has been in use in this hospital for more than two years, and its effects have been the subject of careful observation on the part of Dr. Duncan Hilston, the principal medical officer, and myself, during that time. At his suggestion, I have drawn up the following account of its use, dose, and mode of administration. The views set forth are, therefore, the outcome of our combined observation, and of notes made from day to day in the hospital records. The report will summarise, as shortly as possible, the therapeutic experience of this alkaloid which has been gained, pointing out the dangers and difficulties met with in its use, as well as the good effects which have seemed to follow from its administration. A few typical cases will be selected to illustrate its suitability and action in certain excited conditions of insane patients.

The dose and mode of administration of such a powerful alkaloid first demanded most careful consideration. Numerous notices of the use of hyoscyamine have appeared, from time to time, in the current medical literature; but the dose given or suggested has varied in the most startling way, from one-hundredth to three-fourths of a grain, or even a grain; while important and equal results have been supposed to follow each of these extreme doses in the hands of different prescribers. Then, again, no solution of a recognised and accepted standard strength had been decided on. Every writer tried to find for himself what was most convenient.

The form of the alkaloid used here has been, invariably, Merck's crystalline hyoscyamine. In the earlier instances, it

was used in a solution of one grain in two hundred, administered by the mouth, in gradually increasing doses, beginning with one-hundredth of a grain. No observable effect was produced until one-twentieth of a grain was given at a time, and no very marked effect followed such a dose. It was, therefore determined to adopt a solution of more convenient strength, and one more in harmony with the common pharmacopœial alkaloids —namely, four grains to the ounce. The following formula for its preparation was brought into use, and has been found to meet every requirement as to strength, but lacks stability: Hyoscyamine (Merck's crystalline), four grains; glycerine, distilled water, of each half an ounce; carbolic acid, two minims; dissolve without heat. Dose, four to eight minims, given hypodermically.

It is very important to make the solution without heat, as heat renders the alkaloid nearly inert. Hyoscyamine is a most unstable alkaloid, and soon decomposes; so that the strength of any solution yet devised cannot be depended on for more than a month after its preparation. With this solution, some satisfactory results were obtained; but, before giving one or two instances of this, it may be well to place on record the dangerous experience gained in cases where large doses were given by the mouth. The effects of the hyoscyamine, when so administered, varied much, both as to the time before it appeared, and also as to its intensity and duration. This uncertainty of action was found to depend on the state of the digestion. If the alkaloid were given shortly after a meal, the effect was slight and transitory. Not recognising this, at first, the dose had been gradually increased till half a grain was reached. As patients seemed to bear large doses well, an attempt was now made to prepare a special solution, for hypodermic use, of equal strength with the pharmacopœial hypodermic solution of morphia—namely, five grains to the drachm. A solution of this strength could only be made by the aid of heat; and, as this rendered the alkaloid nearly inert, the attempt was not repeated, especially as further experience showed such a solution to be unnecessary.

Case 1.—An useful lesson was taught by the case of William S., aged 36, a bandsman, who had been an inmate of the asylum for the last eight years, suffering from chronic mania, with occasional outbursts of violence. On March 9th, 1881, he was very violent and destructive, and refused his breakfast. At 8 a.m., he was given one-fourth of a grain of hyoscyamine by the mouth. By midday, his pupils were widely dilated, and he had become quiet. This quiet gradually deepened, though he did not sleep; and by 6 p.m. he was almost powerless, unable to stand without assistance, hardly able to move his

hands or to speak. His breathing was unaffected; but his pulse had fallen from its usual frequency of 75 to 50, and was, in addition, very weak; while the temperature in the axilla was only 96.2°, his ordinary range being 97.6° to 98.4°. His extremities were cold and clammy. His throat, so far as could be made out, was moist; at least, he could swallow with ease. This state of affairs was serious, and the cause of grave anxiety for a time. He was placed in bed, and hot-water bags applied to his feet, and an ounce of brandy with hot water and sugar was given. The extreme effects of the hyoscyamine gradually passed off, and by 10 p.m. his pulse and temperature had again become natural. He slept all night, and awoke in his usual state, except that his pupils remained dilated for forty-eight hours, and during that time he was disinclined for exertion. He has not been violent since, though his chronic mania continues.

In this case, the dose of hyoscyamine was, beyond doubt, too powerful, and was followed by dangerous symptoms of collapse, from which, fortunately, the patient recovered; but the experience gained acted as a grave warning, and called attention sharply to the risk incurred in using such large doses of this powerful alkaloid. As larger doses had been given with impunity, it was assumed that the unusually energetic action of the drug in this case depended on the state of the stomach, as regards digestion, when the medicine entered it. The stomach was empty, as the man had refused his breakfast. This experience, and another similar one, where the symptoms were nearly as alarming, decided the point in favour of smaller doses, given by the hypodermic method.

Case 2 serves to illustrate some points in the use of this drug, and may be taken as a type of patient in whom the gain was marked and immediate. C. C., aged forty, stoker, was admitted on November 4th, 1881, for melancholia. From gloomy and silent depression, he gradually passed into a state of noisy and destructive mania, shouting incoherently, throwing himself off his bed, and dashing himself about, till he was much bruised, in spite of constant care on the part of the attendants. His restlessness was incessant, and he passed several days in succession without sleep. The use of hyoscyamine was begun in this case by the hypodermic injection of four minims (one-thirtieth of a grain) of the solution mentioned above. This was continued twice daily for several days, without any marked effect except that of moderately dilating the pupils. The drug was then omitted for twenty-four hours, at the end of which time his pupils had contracted to their usual size. As such a dose was not sufficient to control or moderate the restless violence of this patient, it was increased to eight minims (one fifteenth of a

grain), given hypodermically at 6 p.m. In fifteen minutes, his pupils began to dilate, and, becoming quieter, he lay down in bed. Just before this, he had been jumping about the ward, resenting and resisting all persuasion to remain quiet or lie down. At 10 p.m. he was sound asleep, his pupils widely dilated. He could, however, be easily roused, but went to sleep again on being let alone, and slept for seven hours, and when he awoke his pupils had begun to contract again. He remained during that day quieter and more easily managed than he had been for a long time. The effect of the drug passed off in about twenty-four hours. We had now got the measure of this patient's power of resistance to the drug, and were thus able to regulate the dose in accordance with his seeming requirements. Often, from a restless, noisy, and destructive condition, he passed, while under the influence of the alkaloid, to a quiet and tractable state, easily managed by day, and obtaining several hours' sleep by night. Indeed, the soothing influence of the drug sometimes lasted for days; but at other times the nervous commotion seemed to get the better of the drug in about twelve hours, in which case an injection of eight minims (one-fifteenth of a grain) was given, night and morning, with the effect of procuring quiet and comfort both to himself and those about him. His excitement came on in periods, and during their continuance quiet and rest were secured by the use of hyoscyamine, without apparent injury to the patient. The drug was omitted whenever the excitement was only moderate; sometimes he would go for days without it, and the dose was occasionally reduced to four minims, when that dose seemed sufficient to mitigate his violence. In this case, the effect of the alkaloid could be depended on and foretold almost with precision, if the drug were given by the skin; while, if administered by the mouth, there was an uncertainty both as to period of onset, duration, and degree of effect. It had, however, no curative power beyond the calming and soothing influence, which no doubt exerted a conservative effect on the patient's strength. As the excitement passed off, calmed and soothed by the drug, sleep seemed naturally to follow; he was disinclined for further effort, and, as in a child tired out with its own boisterous play, mind and body sank to rest.

Case 3.—C. S., aged 48, pensioner, suffering from general paralysis of the insane, was in that troublesome condition of unceasing motor activity often seen in these distressing cases, where they cannot keep still. He was most difficult to manage, full of delusions, yelling to have his head cut off, attacking or making darts at the other patients, pushing about and upsetting the furniture, tearing his clothes to shreds, jumping up the walls to reach the pictures, destroying everything on which he

could lay hands, feet, or teeth; a perfect demon of destruction and unrest. Yet, on standing beside him, gently and firmly pressing him into a seat and remonstrating with him on his conduct, he would be recalled to his saner self for a moment, and, looking up, would seem to recognise for an instant the realities around him, and bursting into tears, describe only too well his own helpless condition in the piteous cry, "I cannot, cannot help it." In a second, he would be off again, and so from day to day. He was calmed and controlled by hyoscyamine; and it is noted that, in twenty minutes after the injection of eight minims (one fifteenth of a grain), from the state above described, his pupils had become dilated, and he allowed himself to be dressed and remained quiet, not asleep, able to take his own food, and, on being placed on the closet-chair, passed urine. It will thus be seen that he was not rendered helpless by the drug, since he was able to feed himself and passed urine on invitation to do so. The effect of the hyoscyamine on the pupils had nearly passed off in twenty-four hours, but he remained quieter and more easily manageable for two days. So far as could be made out, no dryness of the tongue or throat followed such a dose. Often, one-thirtieth or one-fifteenth of a grain was sufficient to calm, control, and soothe this patient when he was otherwise nearly beyond the control either of himself or others. In this case, and in some similar cases, such a dose has been repeated every day, or every second day, for a fortnight, without any observable ill effects, but with great apparent gain to the patients who were, previously to its use, being rapidly exhausted by their restlessness and unceasing exertions.

Case 4.—J. G., aged 34, is an example of its administration in acute mania. This patient was admitted from the Royal Naval Hospital at Plymouth, on May 4th, 1882. Eight men were required to remove him from Plymouth Hospital to the railway station. On admission at Yarmouth, he talked incessantly, raved and stormed in the foulest language. He was full of delusions without system or coherence. "He was a king, a duke, etc. God directed him to fight with people, with every one." And he instantly proceeded to obey the order, attacking every one about him. He was at once given eight minims (one-fifteenth of a grain) of the hyoscyamine solution by the skin at 9 p.m., and in twenty minutes he was quiet, and, with a little pressure, undressed, went to bed, and slept soundly all night. Next morning, May 5th, he was much quieter, and was able to walk in the square with an attendant, but he again became violent, and had eight minims at 11 a.m.; after which he remained quiet till 8 p.m., walking in the square, and quietly talking of his delusions. As he now became violent again the dose of eight minims was repeated, and he fell

asleep in half an hour, sleeping till 2.30 a.m., May 6th, when he started up in terror of being murdered, and attacked those about him. He was partially quieted, and reassured by the presence of the medical officer, who repeated the dose of hyoscyamine at 3 a.m.; this was followed by sleep till 7 a.m. At 11 a.m. the alkaloid was given in a like dose, as he had again become violent. He was then taken into the open air, where he gradually became quiet, and walked up and down with an attendant. He had thus thirty-two minims, or a little over a quarter of a grain of hyoscyamine in twenty-four hours, viz., from 11 a.m. on 5th May till 11 a.m. on May 6th, with the result of calming and controlling his actions and ideas, for the latter even seemed to be moderated by the drug. He took his food well during this time, and did not appear to suffer from dryness of the throat. It was now determined to omit all medicine and try to manage him without drugs, and carefully note the result. No medicine was therefore given during the remainder of the 6th, 7th, 8th, and part of May 9th, and he was allowed to wander about the exercise-ground with every appearance of restraint removed. His violence was, however, often extreme and his language foul, abominable, and blasphemous beyond description; he only slept for a few minutes at a time, while a look of terror and haggard anxiety deepened on his face. On May 9th, at 10 p.m., a hypodermic injection of ten minims of the hyoscyamine solution was given. In half an hour he was asleep; he slept well all night, and awoke with a refreshed look he had not shown for days. As the hyoscyamine appeared to act so beneficially, it was continued from time to time, as his sleeplessness or violence required to be overcome. The dose was increased to fourteen minims on one occasion, when his violence was greater than usual. He had been dashing himself about and screaming for hours, but on this occasion the drug failed of its usual effect, for he still made feeble efforts to shout at or strike those about him though fully under the influence of the drug, as shown by his widely dilated pupils and staggering gait. On this occasion, too, his tongue and throat were dry; but whether this was caused by the hyoscyamine or by his continuous screaming, it is not possible to decide. Each probably had its share. He was still under his delusions, but in a dull, sleepy kind of way. He would start up occasionally as if irresistibly impelled by his mania, yet sinking down instantly, incapable of carrying out the intended action, or, it may be, the idea had vanished ere grasped. Hyoscyamine was finally omitted on May 18th, as he gradually become more manageable, his mania assumed a less pugnacious type, and he slept better at night.

Since hyoscyamine has been brought into use in this hospital,

frequent efforts have been made to note accurately the effects of the alkaloid on the pulse and temperature. No trustworthy observations could, however, be made on patients in the state of those just related. But from clinical observations here, some other practical lessons may be drawn.

1. The observations show the uncertainty of the action of hyoscyamine when given by the mouth, and the danger of large doses.

2. They also show the marked superiority of the hypodermic method, and the confidence with which, in some cases, its effects could be calculated on, and the dose increased or diminished in accordance with the violence of the patient.

3. In hyoscyamine, we have a drug which is often capable of controlling the violence of a furious maniac, and, it may be, checking the torrent of rushing ideas on which he is borne along, soothing without putting him to sleep, and, in these respects, differing from morphia or chloral. In noisy and destructive general paralytics, such as indicated in Case 3, the quiet air of comfort and repose following a moderate dose was such a contrast with the previous condition, as to strongly impress every one with the feeling that, by the introduction of hyoscyamine, another valuable aid had been secured in the care and treatment of such cases.

4. No curative action can be claimed for the drug. Even in acute mania it did nothing more than moderate or check, for a time, the violence of action, and, perhaps, render less vivid and overwhelming the terrifying whirlwind of delusion of the frantic patient.

Previously to procuring a supply of hyoscyamine, the ordinary tincture of hyoscyamus was given in large doses, sometimes as much as one ounce at a time, without much effect, except that of dilating the pupils; but it had no controlling power in cases which afterwards yielded to the more powerful alkaloid.—*British Medical Journal*, *Nov.* 25, 1882, *p.* 1030.

14.—ON NERVE-STRETCHING IN SPINAL MENINGITIS WITH ATAXIC SYMPTOMS, DUE TO INJURY.

By H. G. Armstrong, Assistant-Surgeon, Royal Berkshire Hospital, Reading.

The following case of nerve-stretching, which was done with the idea of relieving pain, may be of some interest. It was one of the first done in this country, and the result has been more successful than that of most cases hitherto published.

Previous history.—G. M—, æt. 40, married, had followed the business of a bricklayer. At the age of eighteen had gonorrhœa and a chancre, but does not recollect having any secondary mani-

festation of syphilis. Has always been temperate and regular in his habits. Has been married twenty-one years, during which time his wife has had seven children, of whom one is now living, three were born dead, and three died within a year of birth. Until his present attack has had good health. Five years ago he fell from a scaffold astride a piece of timber, injuring his lumbar and coccygeal regions, which laid him up for about a week. On resuming work he often had severe pains in the sciatic regions, accompanied by violent tetanic spasms, which prevented his continuing work for a week or two at a time. The attacks of pain and spasm increased in severity and became more frequent, and three years ago he gave up work altogether, stating, as the reason, "that he could not feel his feet when going up a ladder." During these three years he has gradually got worse, and twelve months ago had to take to his bed, from which he has rarely, and for the last four months not at all, been able to get up.

Present condition.—Is a short, thick-set man, with florid complexion and bright, intelligent face. Muscles well developed; skin healthy. Complains of very severe lightning pains, confined principally to the legs and lower parts of the body, though they are occasionally felt in the upper extremities. These pains sometimes last continuously for several days and nights together, but at other times are momentary. Their severity is invariably aggravated by any change of weather, especially during rain or high wind. The lancinating pains and tetanic spasms in the lower limbs can always be produced by pressure on the skin over the coccyx. Does not attempt to leave his bed, as on trying to walk he falls down. Has no difficulty in raising a leg from the bed, but it is thrown violently from side to side, and he is unable to replace it on any given spot. Has entire loss of patellar tendon reflex and complete anæsthesia of both lower extremities up to the groins; sexual power is lost but not desire. Has occasional attacks of gastralgia and disturbances of the bladder and rectum. The electric excitability of the muscles in the lower limbs is partially lost. Vision good, and no cranial nerves seem to be affected.

March 27th, 1881. The patient being held in the lithotomy position, I cut down on the left sciatic nerve, about two inches below the tuber ischii, and holding it between the finger and thumb of each hand, stretched it violently. No anæsthetic was used, and the patient experienced no pain either in the cutting through the skin or while the nerve was being stretched. The wound was stitched up and dressed with thymol lotion (1—8). —28th. Has passed a very restless night. Complains of shooting pains in all parts of the body, more so in the arms and shoulders

than he has ever had them before. Temp. 99.5°, pulse 100.—30th. Still has pain, but not so violent.—31st. Much better, pains very slight. Tactile sensibility is returning, as he can feel a finger drawn over the plantar surfaces.—April 2nd. Has had no pain for twenty-four hours.—7th. Tactile sensibility has returned over the whole of both lower extremities, the prick of a pin being distinctly felt in the thighs.—12th. While lying on his back raised his legs easily from the bed and held them out straight, without any waving from side to side, and had no difficulty in replacing his foot on any indicated spot. No pain.—15th. Has tenderness along the left sciatic nerve, and a return of the severe pains.—25th. Free from pain; is dressed and sitting up, and with very slight assistance can walk round the room. From this time he gradually improved, and on the 12th of May walked out, which he has continued to do regularly. Has return of pain whenever there is a change of wind, but very slight compared with that previous to the operation. Sexual power is restored.—June 24th. This morning walked a distance of seven miles before breakfast without fatigue. Is much improved in appearance, having put on flesh since the operation. Sleeps well, and is seldom troubled with pain, which is only of a trifling character. Spends most of the day in the open air.—Sept. 24th. During the last three months has continued much in the same state with reference to his power of locomotion. Has return of the lancinating pains at intervals. These intervals are gradually becoming shorter, and the pains are again becoming more severe in character, but he still states that they are "nothing like" what they were before the operation. In walking he exhibits the characteristic ataxic gait.—Oct. 7th. No change since the last note.

Although the patient seems to be gradually relapsing into his old condition, a fair amount of success may be claimed for the operation. Before it was done his lower limbs were in such a perfectly anæsthetic condition that he was able to undergo it without the slightest sensation of pain. For twelve months before he had been almost entirely unable to walk, and for three months had been confined to his bed; and the lancinating pains, from which he was never free for more than two hours at a time, were so severe as to make his life a burden to him. The result of the nerve-stretching has been the recovery of cutaneous sensibility, and almost complete relief from the severe pains and tetanic spasms, and after a few weeks the ability to walk from eight to ten miles without fatigue. That this was the result of operating on one sciatic nerve is, I think, a point of much interest, showing that whatever the effect of stretching a nerve may be, it acts in some way, not on the periphery, but on the nerve centre. How the effect is produced

there is no satisfactory evidence, but this should not, I think, be any objection to the operation. Many of our most useful drugs act in a way equally mysterious.—*St. Thomas's Hospital Reports*, 1882, *vol.* xi., *p.* 109.

15.—ON NEUROSES OF THE VAGUS.

By Dr. L. Kredel (in *Deutsch. Archiv für Klin. Med.*).

Dr. Kredel discusses this subject at some length with reference to observations made by him in the medical clinique of Professor Riegel in Giessen. He points out that, although the effects of injury or pressure upon the vagus nerve have been much studied by means of experiments upon animals, there is a manifest want of confirmation of the results so obtained, from a purely clinical standpoint. The most obvious results of injury to the vagus, in clinical experience, are acceleration of the heart's action and paralysis of a vocal cord. The symptoms referable to abdominal organs are too uncertain for recognition. The effects upon the respiration would appear to be almost contradictory to those obtained by experiment upon animals. The slow and deep inspirations thus produced have been but seldom observed (a few such cases are, however, recorded by Guttmann); the results of injury to the nerve being generally, in the case of the lungs, entirely negative.

That the nerve plays an important part in the phenomena of asthma has long been recognised, and Biermer has of late called especial attention to the production of acute emphysema by the resistance offered to the expiratory efforts by the bronchial spasm. For certain forms of angina pectoris, the agency of the vagus nerve must be held responsible, especially in those cases where no definite organic changes are present; although other agencies also must be recognised, more especially that of the vasomotor system and the automatic action of the complicated system of centres within the heart itself. The majority of opinion amongst writers upon this subject regards the disturbance of respiration in angina pectoris as due entirely to the pain and anxiety produced, although a few have described true asthmatic paroxysms. It is probable that a certain amount of dyspnœa is nearly always present; but the fact that in some cases the respiration is not interfered with, is in itself a proof that the disturbance of the heart's action is not a sufficient explanation of the dyspnœa when it does exist. A certain group of cases, in which dyspnœa has accompanied the ordinary manifestations of angina pectoris, has furnished at least one explanation, viz., the rapid production of acute emphysema, which may as rapidly subside with the conclusion of the paroxysm. A case is related in which sudden palpitation was

followed by intensely rapid heart's action (198-208), and a remarkable extension downwards of pulmonary resonance without very marked dyspnœa, although the respirations became more frequent. In twenty-four hours the pulse became normal again, and in thirty-six hours the lung resonance had returned to its former limits. Some relation evidently existed between the accelerated cardiac action and the acute distension of the lung. In a second case, although the disturbance of the heart's beat and of the normal limits of pulmonary resonance were even more marked, there was again no excessive dyspnœa. Considerable cyanosis, however, was observed, and distinct evidence of congestion within the lungs appeared. All these symptoms became developed with great rapidity, and as quickly subsided in the course of a few hours.

In this case, a double cause for dyspnœa may be assumed. The mechanical narrowing of the tubes by congestion is added to the spasmodic contraction of their muscular fibres, assumed in the previous instance. In a third case, of less intensity, the disturbance of respiration ceased earlier than that of circulation. Such cases, presenting the features both of angina pectoris and of asthma, might well be described as examples of 'cardiac asthma.' These cases belong essentially to the group of neuroses of the vagus; the characteristic cardiac phenomena must be regarded as due to an intermittent disturbance of the functions of the cardiac inhibitory fibres. The acute emphysematous condition is, however, produced by spasmodic contraction of the smaller bronchi, and this in its turn by irritation of the pulmonary fibres of the nerve. If the cardiac and pulmonary symptoms own the same peripheral cause, it is necessary to assume the existence of simultaneous irritation of the pulmonary fibres and paralysis of the cardiac fibres as they pass along the trunk of the nerve.

In relation to the first of these cases, Tuczek accepts this explanation, and calls attention to the greater susceptibility of the cardiac fibres to irritation, and also to the unequal distribution of the same fibres in the two nerves. He assumes the compression of the nerve within the chest by enlarged bronchial glands, as the most probable cause of the condition. Although the phenomena observed may be fully accounted for in this way, still the explanation appears somewhat forced, and another interpretation of the sequence of events suggests itself from the well-known experiments of Hering, in which, by forcible distension of the dog's lung, he produced diminished blood-pressure and accelerated heart's action, which continued only so long as the pressure within the lung was maintained. By energetic distension of the pulmonary tissue, he increased the number of cardiac pulsations threefold. Going further, however, he found

that extreme pressure again depressed the number of pulsations. Continuing his experiments, he found that this accelerated heart's action was not due to the influence of the expanded lung upon the heart itself, nor did it depend upon the altered conditions of circulation within the lung, but that it was clearly induced by reflex influences conveyed by the vagi nerves. The irritation of the sensitive nerve-fibres within the lung being referred to the inhibitory cardiac centre, the action of the inhibitory fibres became to a certain extent suspended.

His explanation of the phenomena observed in these cases would, therefore, run thus. From whatever cause, as, for instance, rapidly enlarged bronchial glands, the pulmonary branches of the vagus become irritated, spasm of the smaller bronchi takes place, increased pressure and consequent distension follow. The irritation of the sensitive fibres thus set up is reflected to the centre, and in this manner the accelerated pulsation is induced.

Dr. Kredel concludes with the observation that, whatever detailed explanation may be accepted, there can be no doubt that the whole series of phenomena in such cases must be regarded as due to a neurosis of the vagus. They can neither be classed with bronchial asthma nor with angina pectoris, and must, therefore, be separately distinguished as cases of cardiac asthma.—*Dr. E. Clifford Beale, London Medical Record, Nov.* 15, *p.* 446.

16.—ON ACTIONS OF REMEDIES, AND ON EXISTENCE OF NERVES OF INHIBITION AS EXEMPLIFIED BY THE ACTION OF SEDATIVES AND STIMULANTS.

By Hugh Owen Thomas, M.R.C.S., Liverpool.

The action of physiological doses and the probable effect of therapeutic doses of many remedies have, in most instances, been deduced from observing the effect of lethal or toxic doses. Such conclusions are not trustworthy evidence of the remedial qualities of drugs, inasmuch as when the lethal condition is approached the distinguishing signs of special poisoning begin to merge, so that their identity is nearly lost. For instance, the differences between a fatal dose of belladonna and one of opium or strychnia, are less than the variations of symptoms to be noticed when the subject is under a safe or physiological dose of either of these. The effect of the fall of a balk of timber, sufficient to kill, on one person would give no information to a witness as to what would follow if there descended on another person a portion of timber too light to kill. Again, even conclusions arrived at after witnessing the action of toxic doses have been tinged by our previous opinion of their quali-

ties. This antecedent bias has caused recent investigators to assert the possibility of certain medicines possessing, in varied doses, diverse properties—stimulating one, and depressing at the same time another structure. This error has arisen from not giving sufficient attention to the fact, *that each drug has a primary affinity for certain structures, thus causing a temporary defect of co-ordination.*

Some writers on therapeutics have made a class distinction between sedatives and narcotics, this classification being based upon the various affinities of certain drugs for particular structures. I fail to see that this is justification for separating those drugs that have been termed sedatives and narcotics. To me the terms are synonymous. To place various drugs in diverse classes because they may vary in affinity for separate structures is as reasonable, as to vary the species of the different members of the human race, on account of the quality of the food they incline to. Sedatives or narcotics retard life, and their effect upon the structures, which they primarily operate upon, *is to inhibit more or less their function and to cause in other structures, unaffected by the sedative, the signs of defective inhibition or want of co-ordination,* identically the same signs observed after mechanical interference with such structures.

In experiments performed upon the vagus nerve, all mechanical interference, such as section, ligature, and electric shock, has been termed stimulation or excitation of the nerve. This is, in my opinion, incorrect, as either of these gives rise to a shock to the nerve, arresting its action. These experiments have also shown that the nerve is capable of acquiring some degree of habituation, so that the shock from mechanical interference loses its effect, just what we observe to follow in the use of drugs. In proof that mechanical irritation of this nerve induces a condition of shock, we have the fact that atropia (true stimulant) protects the nerve from the shock consequent upon mechanical disturbance. I have not as yet met with any evidence which proves the existence of any inhibitory nerve fibres in this or any other nerve.

Again, diverse qualities have been attributed to drugs from observing their mode of action varied upon the lower animals as regards symptoms in comparison with the signs of their action on man ; but this fact does not inform us that any drug varies in its properties, whether given to man or any of the lower animals. It only demonstrates that drugs vary in their affinity for analogous structures in the various types of animals experimented on ; although one drug may give rise to varied degrees of intensity of symptoms in the several types of animals tested, yet, in all, the ultimate drug effect will be found to be identical.

Do sedatives act as direct stimulants?—I believe they do not; but their primary effect may be to simulate stimulation, and in those instances where this simulation appears it is a primary action — then, also, the sedative is exerting a minimum or medium effect only. Opium and alcohol belong to the class of pure sedatives, and their action upon the several organs of the body confirms this. Their effects can be best observed by noticing their physiological influence upon the iris, heart, blood-vessels, and viscera. If a full dose of opium, short of being a rapidly fatal dose, be given, the diameter of the pupil becomes diminished. This is caused by the drug having a primary sedative or paralysing action upon the radiating muscular fibres, through its primary affinity for the sympathetic system of nerves specially controlling the radiating fibres of the iris. But if a fatal dose be administered, then the cerebro-spinal system of nerves, hitherto less affected by the opium, shows signs of its full toxic effect, and the circular muscular fibres of the iris also become paralysed, as evidenced by the increased diameter of the pupil. The effect of opium upon the heart and blood-vessels is to act first upon the blood-vessels, but later on the heart secondarily. Hence we have at first an increased volume in the pulse from diminished tonicity, and finally a slower rate of beat when the dose has been sufficient and has had time to influence the heart. There is also to be observed a diminution of the solid constituents in the liquid secretions of the body and a fall of temperature. All these are signs of retardation of vital changes — sedative action. The exception to these general signs of the physiological effect of opium is to be met with when small initial doses of opium are given; then may be noticed acceleration of pulse and vomiting, which may be thought to indicate stimulation rather than retardation.

In explanation of this clinical fact, which appears to disprove my contention, I advance the following reasons:—(1) This simulated stimulation is only temporary, and is evidence that the drug has affected only those structures for which it has a primary affinity—the time being too short or the dose too small for its full physiological action to have been developed; and thus the phenomena of the so-called defective inhibition or want of co-ordination appear—this is often interpreted as indicating stimulation. (2) That by the use of any remedy there is introduced into the system a foreign body, which may give rise to some temporary constitutional disturbance until some amount of habituation has been acquired. We have many familiar examples of this — as change of air, diet, pleasure, relief of pain, sea voyage—yet no physician would advise a trip to sea in place of prescribing an emetic, the latter

being nearer at hand and more certain of action. So with opium, its indirect effect in simulating stimulation is not so ready or so safe as employing a genuine stimulant, when the effect is desired. (3) Some remedies are, at times, modified in composition by the condition of the secretions which they become mingled with after introduction into the body—this change of character being frequently influenced by the age of the patient. Any experienced practitioner must know how rarely any of the signs of so-called stimulation comes on after the administration of opium to subjects under the age of ten years; yet how frequently are we disappointed in its action when given to patients who have passed the meridian of life, the intestinal secretions then being in many instances probably abnormal—for, if the remedy be given by the skin method in preference to the mouth, an unalloyed sedative effect follows. (4) The most probable explanation of the non-occurrence of vomiting after the administration of opium I believe to be that in some subjects, especially children, it rapidly affects the pneumogastric nerve and its branches, so that defective co-ordination is avoided, and thus the stomach and intestines remain quiescent. In proof of this there are the observed clinical facts that only large doses produce vomiting at the commencement of their action, or at the termination of their action, *i.e.*, when the pneumogastric nerve has not been yet reached by the drug or its influence on the nerve is waning, this nerve being affected later and recovering earlier from the drug than the sympathetic nerves. There is further proof in the fact that when opium is given by the skin method its action is rapidly operative all round, and the period of possible and isolated excitement of the pneumogastric nerve and its branches is bridged over so that vomiting is avoided. This explanation of the phenomena of vomiting after the use of a sedative is quite consistent with what we observe of the effect from doses of belladonna, which also induces vomiting occasionally.

Many of the prevailing errors regarding the therapeutic effects of both opium and alcohol have arisen from misinterpretation of the signs of their action, and of the symptoms of the disease which they were required to correct.

Alcohol in its various forms, as in popular use, I maintain to be a drug possessing purely sedative properties, and in its method of action is allied to opium. It primarily affects the sympathetic nerves, then the vagus, and finally the cerebro-spinal system. When the nerves become subject to a full non-toxic dose, the pupil contracts; but soon after a fatal dose has been taken, the pupil dilates some time before death. The action of alcohol upon the heart and blood vessels, through the

vagus and sympathetic system, is also analogous to that of opium. Primarily it attacks the blood-vessels through the sympathetic nerves, diminishing their tonicity,—thus relieving the heart from blood pressure,—so that the initial signs of its action may be a temporary acceleration of the pulse, as well as an increase of its volume, simulating stimulation; but if the dose is sufficiently increased, then the heart is also affected, and the pulse becomes reduced in rate.

Again, by alcohol, the solid constituents of the liquids secreted are diminished, and the normal quantity of carbonic acid exhaled by the lungs is reduced; and in corroboration of these ascertained data there is to be observed a fall of temperature. All these signs point to a purely sedative result—retardation of life, no acceleration or stimulation. The simulated signs of stimulation by alcohol arise from the primary affinity that certain doses of alcohol have for certain nerve structures, and a misinterpretation of the signs of its primary action has engendered the belief that true stimulation is gained; and though sometimes this mistake in practice may do no harm, yet when true stimulation is required its administration would be a serious error. Its primary effect can not be a safe substitute when stimulation is urgently demanded and requires to be continued. It may be argued that contraction of the pupil, when influenced by alcohol, may be brought about by stimulation of the circular muscular fibre of the iris, and not by paralysis of the radiating muscles; but the only explanation admissible regarding the mechanism of the increased volume of the arteries when influenced by alcohol, enables us to check our deduction regarding the mechanism of the action of the iris under its influence. If this drug could stimulate, the diameter of the blood-vessels would be lessened from contraction of their circular muscular coat. Again, if alcohol could stimulate, then its primary affinity for structures specially under the control of the sympathetic would cause this stimulating property to influence first the radiating fibres, and dilatation would be the first alteration observed in the pupil during its first stage of action.

Calabar Bean.—Other neurotic sedatives act, after introduction into the human frame, much like opium and alcohol. I have observed the action of Calabar bean frequently during latter years, when prescribing it for chorea, tetanus, and the muscular spasm attendant upon fractures of bones. I have observed that its action has many signs in common with opium and alcohol. In the early stage of its action the sympathetic nerves first begin to feel its effect, and we have vomiting and purging, from its delayed effect upon the pneumogastric branches of nerves supplying the muscles of the intestinal muscular coat; but as soon as the sympathetic nerves succumb to its influence, then the cerebro-

spinal nerves are inhibited also, and the striated muscles relax. The heart is nearly as much inhibited by this drug as it is by digitalis, but its sedative action upon the heart is greater than that of opium or alcohol. During the physiological action of Calabar bean the pupil becomes contracted from palsy of the radiating muscle of the iris, but as soon as a lethal dose is operative the pupil before death dilates, showing that other nerve centres have been influenced. Henbane, another drug of the sedative class, during its primary action dilates the pupil, and this is explicable by the fact that henbane possesses a primary affinity for the cerebro-spinal nervous system. Experiments have shown that the striated muscles are first controlled by it; secondly, the sympathetic; and finally the vagus becomes inhibited, so that the pulse, accelerated during its primary action, is finally reduced below the normal rate.

Digitalis, again, is a sedative that possesses a primary affinity for the vagus, and is practically useful in influencing the important organs to which the nerve is distributed. During its primary or physiological action it has no effect upon the muscles of the iris, and the anatomy of the nerve, which digitalis primarily affects, excludes the probability of the iris being influenced until a lethal dose of the drug has been taken and other nerve centres have become poisoned by it, then the pupil dilates before death. When the merits of various anæsthetics are discussed, ether is frequently incorrectly referred to as a heart stimulant, when the proper explanation of its merit should be that its affinity for the heart, probably through the vagus, is less than that of chloroform, and from this it is a safer anæsthetic. In the action of septic poisons there are examples of special affinity for various structures, these so operating that the so-called signs of defective inhibition or want of co-ordination become the distinctive signs of special diseases.

In the medical practices of the future, this selective affinity for certain structures, possessed by contagious and infective poisons, will become a basis for the selection of aids in treating the diseases which these poisons cause, in place of the present tendency to attempt to neutralise the original evil by a general antiseptic saturation of the blood and tissues, that in future we shall have more of physiology and pathology, and less of chemistry, to guide the physician.—*Medical Press and Circular, March* 14, 1883, *p.* 225.

17.—USE OF APOMORPHIA IN CASES OF POISONING.

By Amand Routh, M.D., B.S., M.R.C.P.Lond.

Those liable to be called to cases of poisoning are always glad to have an agent handy which, not in itself lowering, will

produce prompt emesis, especially in those cases where the jaws are rigidly clenched and the stomach pump absent or inadmissible. This agent I am sure we have in apomorphia, an alkaloid which Dr. W. Murrell has brought before the profession. Though a derivative of morphia, it has no narcotic effects in the doses required to cause emesis. Dr. Murrell recommends it to be kept in a solution of 1 in 50 strength, and to be given subcutaneously in doses of from $3\frac{1}{2}$ to 10 minims ($\frac{1}{75}$ to $\frac{1}{5}$ grain). Emesis occurs in from two to five minutes, the contents of the stomach being usually voided in one rush without previous nausea, but with violent and visible muscular action of the stomach walls. The following two cases will serve to show its utility.

Case 1.—I was sent for to see Mrs. S——, who was said to have swallowed a white powder and to be then dead. I found her on the floor, doubled up, jaws and hands clenched, blood and froth at mouth, respiration seemed absent, and pulse barely perceptible. She had not vomited. Though evidently dying, I injected five minims of the above solution into her arm, keeping my hand on the pulse. In two minutes and a half by the watch the stomach evacuated its contents with a rush, whilst the pulse seemed to rally for an instant and then finally ceased. Oxalic acid was proved to have been the poison used, and at the post-mortem about two drachms only of fluid were found in the stomach.

Case 2.—A lady, a dipsomaniac, had obtained access to the wine-cellar and had swallowed straight off two bottles and a half of brandy. She then put the corks in her pocket, hid the bottles, put on her clothes, and went out for a walk with her footman. She walked quite steadily for 300 yards, when she dropped down insensible, and was carried home in a cab. On arrival, ten minutes after, I found her comatose, not able to be roused, respiration stertorous and infrequent, pupils dilated and insensible, jaws clenched, pulse slow and intermitting, two or three beats in every eight. Her stomach was full of fluid. I injected $3\frac{1}{2}$ drops of the solution, and in exactly three minutes and a half about a pint of alcoholic liquid was expelled, and altogether in about five minutes a quart (measured) of hardly-altered brandy was vomited. The pulse and respiration now improved, the pupils becoming slightly sensible, and I left her for two hours, by which time she could be roused temporarily. After twelve hours' sleep she awoke none the worse.

Apomorphia fails to cause emesis during chloroform narcotism, but no other drug seems to be antagonistic to it, and there is no reason why it should not be used to get rid of even morphia itself. In the dyspnœa of chronic bronchitis, emesis

from apomorphia produces temporary relief. If only the certainty, rapidity, and absolute safety of apomorphia were known, it would undoubtedly form part of every practitioner's paraphernalia.

[Dr. Routh has since (Lancet, Dec. 30) received a note from Dr. Murrell stating that it was Dr. Gee who first introduced the use of morphia to the profession.]—*Lancet, Dec.* 23, 1882, *p.* 1073.

DISEASES OF THE ORGANS OF CIRCULATION.

18.—ON THE USE OF NITRITE OF SODIUM IN THE TREATMENT OF ANGINA PECTORIS.

By MATTHEW HAY, M.D., Demonstrator of Practical Materia Medica in the University of Edinburgh.

It is now nearly two years since I procured a small quantity of nitrite of sodium with the object of trying it as a substitute for nitrohydrochloric acid in the treatment of certain forms of liver disease. In order to ascertain to what extent it might be safely administered, and whether it possessed any untoward action, I took on three separate occasions five, ten, and twenty grains of the salt. I then observed that the rate of my pulse became accelerated shortly after taking each dose, and most distinctly after the largest dose. But, what was more remarkable, I experienced within a few minutes after taking the two larger doses a feeling of fulness in my head and eyes, accompanied by a throbbing sensation. There was also a slight, almost doubtful, flushing of the countenance. The sense of fulness and throbbing continued for an hour or more after the administration of the salt, without at any time being so intense as to be unbearable, or even severe enough to prevent me from proceeding with my usual duties. Indeed, it was comparatively trifling, and caused me no inconvenience. The smallest dose of the salt produced a similar effect, but of very short duration and very slight in degree—so slight as almost to have escaped observation. These experiments were repeated on myself and a few of my friends, and always with the same result.

The similarity of the effects of nitrite of sodium, although less in degree, to those produced by nitrite of amyl and nitroglycerine at once suggested that the peculiar action of these bodies was dependent on the nitrous acid present in them. The suggestion occurred the more readily, as I was previously aware that Gamgee had shown that the chemical action of nitrite of amyl on the blood was identical with that of other and simpler nitrites, as, *e.g.*, the nitrite of sodium. And it is

to its action on the blood that many investigators ascribe the physiological effects of the nitrite of amyl. Moreover, it appeared highly remarkable that two salts, as nitrite of amyl and nitro-glycerine, whose bases differ so widely in their pharmacological activity, should so exactly agree in their physiological action and therapeutical applications, were it not that the acid, which is common to both of them, was the essential factor in the production of their action. As these substances had been found, the former by Lauder Brunton, and the latter by Murrell, to be of the greatest value in the treatment of angina pectoris, I thereupon resolved to try the therapeutic effect of nitrite of sodium in the first well-marked case of angina which might come under my care. Such a case did not present itself until November of last year. Since then, I have searched the literature of pharmacology to ascertain what, if anything, has been written on the action of a simple nitrite, or a nitrite whose base possesses, at any rate in small doses, little action of its own. My search has not been fruitless; for I have found that, in addition to Gamgee's paper and various other publications on the action of amylic nitrite and other ethereal nitrites, there exist accounts of three researches on the action of metallic nitrites by Barth, Binz, and Reichert and Weir Mitchell respectively.

The only case of angina pectoris in which I have as yet used, and, I am happy to say, with success, the nitrite of sodium was a well-marked case of this disease. T. B., aged forty-two, married, a letter-press printer, consulted me on the 15th of November, 1882, complaining of severe and prolonged spasms of pain in his chest occurring several times a day, and even disturbing his rest at night. His father died suddenly of heart disease at the age of fifty-two, but never suffered from pains in his chest. His family history is otherwise unimportant.

Until within the last year, the patient does not recollect of having any illness except the usual fevers incidental to childhood. His father told him that he was a difficult infant to rear, that he was late in learning to walk, and that shortly afterwards he lost the power of walking, which he did not recover until after several months. He was apprenticed to printing at the age of eleven, and six years later began to work a hand-press, which he continued to do for nine years, and which required great bodily labour. Ever since then he has worked at a steam-press, where the exertion needed is much less. About six and a half years ago, whilst doing some strong and sudden pulling in connection with the press he racked himself, and for some weeks afterwards had a peculiar feeling about the epigastrium and substernum as if the

stomach were pressing upwards. Two years ago, whilst dancing, he felt a peculiar pain in his chest, which passed off tolerably quickly and did not again recur. There is no history of rheumatism, acute or chronic. He is a man of fair muscular development, and his expression is careworn and anxious.

The patient attributes the commencement of his present illness to an excessive indulgence in both food and stimulants at a dinner with some friends on the 1st of January 1882. He felt very sick and vomited next morning, and continued to feel sick in the morning and vomit occasionally for nearly three months afterwards. During all this time he suffered from a constant pain, often very severe, in the region of the stomach; and for the purpose of relieving the pain he used to take from one to three glasses of whiskey in the course of the day. But finding that the pain began to get worse, he consulted his usual medical attendant, who, of course, ordered him to abandon stimulants, except a glass or so of beer in the week. With this important modification of his habits, and with the use of the medicines prescribed, in the course of other two or three months he felt considerably better, and the pain had almost entirely gone. About the middle of June, however, whilst walking hurriedly along the street after dinner, he felt a peculiar and severe pain at mid-sternum, quite different in character and situation from the pain he had formerly experienced. The pain gradually went off as he continued walking, but it recurred in the evening whilst walking up hill home from his work. I may here mention that the patient resides fully half a mile from the printing-office where he is employed, and that he has to mount one long and steep hill on the way from the office to his house. At this time it was his custom to walk home for both breakfast and dinner. Until the end of June he repeatedly felt this peculiar pain in his chest, generally on walking up the hill on his way for breakfast and dinner and in the evening, and frequently also on returning to his work after dinner. The pain was gradually getting more severe, and shortly after the beginning of July he began to experience it after rising in the morning as soon as he commenced to put on his clothing, and also on his way to his work in the morning. Indeed, a small amount of exertion at any period of the day seemed sufficient to initiate it. By the end of July, or nearly six weeks from the time he first observed it, the pain had become so severe and actually excruciating that, whenever it occurred whilst he was walking in the street, he was obliged to stand for a few minutes until it became less severe, and sometimes he had to stop almost every twenty yards on his way up the hill. He now ceased going home for dinner,

and a week or two later had also to give up going home for breakfast. But the spasms of pain continued to recur as strongly as before, whenever he attempted to walk a short distance or when he exerted himself unusually at his employment. A tolerably severe spasm was also felt after dinner, without the patient having exerted himself in any way. This was his condition, only that the pains were still more severe and more frequent in their occurrence, when he came to consult me on the 15th of November last. He was quickly losing flesh and strength, and was barely able to continue at his employment. He had been prescribed for by more than one physician, receiving at one time ethereal stimulants and hyoscyamus, at another time digitalis and iron, without deriving the slightest benefit. On his own responsibility, besides a selection of patent medicines, he had tried large doses of quinia for a time, but also without effect. Sometimes, with the object of relieving the pain, he took a glass of whiskey, sometimes a glass of beer, and occasionally both together; but whilst they questionably helped to deaden the pain, they did not prevent its returning shortly afterwards with as much severity as ever.

On the 15th of November, when I first saw him, the following was the condition of his circulatory system. The radial pulse was fairly strong, moderately full, regular in force and frequency, and eighty per minute, and there was no perceptible rigidity of the radial or any other superficial artery. The heart was slightly hypertrophied, and its impact against the chest-wall at each pulsation was somewhat stronger than usual. Both of the heart-sounds, as heard at the apex, were soft and prolonged; in the aortic area the first sound was followed by a short, soft blowing murmur, and the second by a louder and more prolonged murmur of the same character. There was evidently a lesion of the aortic valves.

The patient described each attack of the pain as beginning in the middle of front of the chest and extending over a space of about the size of the hand. It gradually extended backwards until it was distinctly felt between the shoulder-blades, and it passed down both arms even to the finger-tips, and in the arms was particularly severe in the wright wrist. He was generally made aware of its approach some one or two minutes before it was actually felt by a peculiar, uneasy sensation in his chest. The pain, once it had commenced, lasted generally for nearly fifteen minutes, but sometimes for five or ten minutes only, and was quite excruciating. Just before the pain began to abate, the patient felt a throbbing between his shoulders at the back of his chest. The pain passed off quickly and entirely. Whilst it lasted he experienced no sense of suffocation or difficulty in breathing, no sickness, and no giddiness. I examined

his urine, and found that it was natural and contained no albumen or sugar. There was no arcus senilis, and there was no dropsy of any kind. The pain from which he suffered was evidently a well-marked angina pectoris, as we commonly understand it.

The treatment I recommended was twofold—for the angina, three minim capsules of nitrite of amyl to be broken and the contents inhaled whenever an attack supervened or was impending; and for the removal of some gastric irritation, of which there was still some evidence, and which probably arose from chronic gastritis, I enjoined regulation of diet, abstinence from all alcoholic stimulants, and the use of a sedative mixture which I prescribed for him. He called again on the 30th of November, looking and feeling much better. The gastric irritation was gone, and the pain had been greatly relieved by the nitrite of amyl. He had used the capsules at the rate of six or seven a day, indeed, whenever he felt the pain commencing—one in the morning immediately after rising, another on his way to his work, a third after, and sometimes before dinner, a fourth before leaving for home at night, which enabled him to walk home without suffering any pain, and a fifth at bedtime, with an occasional one during the night; for, latterly, he had begun to have one and, generally, two attacks of the pain during the night. Besides these, there were other occasions, as when working more actively than usual, or when out walking, that necessitated his using the capsules. The inhalation of the nitrite did not entirely dispel the pain, it merely dulled or deadened it, and it was always accompanied by giddiness, which compelled him, if he were at the time in the street, to stand for a minute or two, and it was followed by a headache and disagreeable feeling which lasted for one or two hours. He was, however, extremely grateful for the relief from severe pain which the nitrite of amyl had been the means of giving him. It was at this time that, recollecting what I had some time ago thought as to the action of nitrite of amyl being for the most part due to the nitrous acid, I resolved to make a trial of nitrite of sodium, which, if it were followed by satisfactory results, would offer therapeutical proof of the correctness of my supposition, and at the same time furnish, in addition to nitrite of amyl and nitro-glycerine, for the treatment of a distressing disease, a valuable remedy, inasmuch as it contained the nitrous acid in its simplest possible combination, uncomplicated by the presence of a base possessing an undesirable or disagreeable action. I therefore gave my patient the following prescription: ℞. Sodii nitritis, oz. ½; aquæ ad fl. ozs. xii. Solve. Sig. Dose, one or two teaspoonfuls. As I did not feel assured that the nitrite of sodium would prove serviceable, I asked him also

to procure some nitrite of amyl, but in bulk, and to inhale five or six drops of it, as I was under the impression the quantity in each capsule was not sufficient to produce the desired effect. The expense, too, of the capsules, where so many were required, formed a consideration. The nitrite of amyl was only to be resorted to in the event of the nitrite of sodium proving inefficient.

The patient returned on the 8th of December with an extremely favourable account of the benefit he had derived from the use of the nitrite of sodium. He had taken, as I had suggested, a dose a few minutes before rising in the morning, and the result was that he was enabled to get up, dress, breakfast, and walk to the printing-office, without experiencing the slightest pain, and without requiring more of the nitrite until after dinner. He tried to do without it one morning, but the pain at once came on when he commenced to dress himself. After dinner he took it only when he felt the pain was coming on, and if he took it promptly it caused complete cessation of the pain in from one to two minutes. Nitrite of amyl never completely abolished the pain, not even when he inhaled from ten to twenty drops of it. It is possible, however, that the preparation he obtained in bulk was not good, for even one of the three minim capsules which he formerly used had a stronger effect than twenty drops of it, although in no instance had the nitrite of amyl given him that complete freedom from pain which the nitrite of sodium did. Another important difference, he said, was that the nitrite in the dose prescribed caused no perceptible throbbing in any part of the body, and certainly no headache. Apart from its effect on the pain, it seemed, he added, to have no more action than so much water. If he required to take it in the street, he could do so without having to stop for a few minutes afterwards and without feeling in the least giddy. In addition to taking it in the morning and after dinner, he took a dose always before leaving for home at night, and another at bedtime. He still continued to waken once or twice during the night with the pain, which a dose of the nitrite of sodium at once arrested. He felt very greatly pleased with his new remedy, for not only was it preventing or relieving the pain, but his general health was improving under its use (perhaps because it kept him free from frequent, almost intolerable, pains), and he felt fitter for his work than he had done for nearly two years previously. Certainly his general appearance was greatly improved.

The next time I saw the patient was on the 21st of December. His general health was still improving, and the nitrite of sodium still continued to completely relieve, or rather prevent, the spasms of pain. Whenever he omitted to take the nitrite

he found that he was liable to attacks of the pain almost as severe as before he came to consult me. If he took none of the medicine immediately after rising, he never failed to have an attack of pain when he commenced to dress himself, and another on his way to his work.

In order to ascertain if the effect of the nitrite of sodium was more prolonged than that of the nitrite of amyl, I asked him to again procure some capsules of a pure preparation of the latter, and on four consecutive mornings to alternately take a dose of the former and inhale the contents of a capsule of the latter, and to observe within what time in each instance an attack of pain followed. The medicine was to be used immediately before rising, and before any symptom of pain was experienced. At the end of the fourth day he returned to report that whilst on each morning in which he had inhaled the nitrite of amyl he was able to dress and breakfast without suffering from the pain, yet on both occasions a severe attack of it supervened after breakfast, when on his way to the printing-office.

On the other hand, a single dose of the nitrite of sodium had kept him perfectly free from pain until after dinner—that is, until between two and three in the afternoon. The preventive action of the nitrite of sodium is, therefore, exercised over a much longer period of time than that of the nitrite of amyl.

I next asked him to compare the two as to the power and rapidity with which each of the two medicines arrested the spasm of pain, if the medicine were not taken until the fit of pain had commenced, or had given indications of its supervening. After having compared them in this respect more than once, he came back to tell me, that, as I had expected, the nitrite of amyl acted more promptly than the nitrite of sodium, but that, in the doses taken, the latter, when once its action had asserted itself, which occurred in from two to three minutes, completely relieved the pain, whereas the former merely dulled the pain, and did not shorten its duration. The nitrite of sodium acted more quickly than I anticipated, and when taken, as he informed me, during the monitory stage preceding the actual onset of the angina, the pain was prevented from occurring.

The next occasion on which he came to see me was on the 6th of January of the present year. His general health had been steadily improving, and his appetite for food and his capacity for work had not been better for several years previously. The pains were still continuing to be felt unless the nitrite of sodium were taken, but they were not quite so severe, nor did they last so long as formerly. At the commencement of the year he had resumed his old custom of walking home for his

dinner, and by taking a dose of the medicine immediately before starting for home, and sometimes a little immediately after dinner, he was able to accomplish the walk home and back in perfect comfort. He still continued to be awaked by an attack of the pain during the night, but now never oftener than once, and generally between 3 and 4 a.m. During the day he took the nitrite almost as frequently as before, and at the times I formerly mentioned, with an extra dose on any occasion when, from having to do his work more hurriedly than usual, or after running upstairs, he felt that the pain was about to come on. On one or two evenings, as I had advised him, he took at bedtime a large dose—three to four teaspoonfuls—of the medicine, in order to try if it would altogether prevent the occurrence of the pain during the night, but although each dose produced immediately after being swallowed a throbbing through his whole body and a dull beating at the vertex of his head, yet he was awakened as usual by the pain about 4 a.m., when he had to take another dose of the medicine. Beyond the throbbing and beating, the large doses caused no other inconvenience; there was no headache or pain, and only a very slight giddiness. It may be as well to mention that the patient, in taking an ordinary dose of the solution of the nitrite, never measured it exactly, but swallowed a small mouthful from a vial of the solution which he always carried with him. By ascertaining how many doses he was able to get from the bottle, and the capacity of the bottle, I calculated that each dose corresponded very closely to two teaspoonfuls, or between four and five grains of the salt. In order to observe roughly the physiological effect of such a dose on the patient, I asked him to take one in my presence.

2.55 p.m. Pulse, 85.	3.4 p.m. Pulse, 84.
2.57 p.m. ,, 84.	3.6 p.m. ,, 89.
3.0 p.m. Took fl. ʒ ii. of solution of nitrite of soda.	3.12 p.m. ,, 88.
	3.17 p.m. ,, 84.
	3.20 p.m. ,, 84.
3.2 p.m. Pulse, 86.	

Before taking the nitrite, the pulse was firm and regular, and tolerably full. After taking the nitrite, it became a little more rapid, and seemed to feel slightly, almost doubtfully, softer and smaller to the touch, but was certainly not greatly altered in character. There was no appearance of flushing, and the patient felt no throbbing and no unpleasant sensation. Immediately afterwards, I took the same dose of the same solution myself, but without observing any effect. The pulse was altered neither in character nor in frequency. It is certainly remarkable that a dose of the nitrite of sodium which produces so little apparent

physiological effect should so thoroughly relieve the patient from an excruciating pain. On this occasion, for the sake of comparison and without saying anything whatever as to its probable action, I asked the patient to try nitro-glycerine for a few days instead of nitrite of sodium, and I prescribed it for him in the following simple form: ℞. Sol. nitro-glycerini (1%) fl. ʒ i.; aquæ ad fl. ℥ vi. Sig. Dose, one to two teaspoonfuls.

He returned four days afterwards to tell me that the new medicine, which he had used to the entire exclusion of the sodium salt since I previously saw him, acted quite as well as the latter, completely preventing or relieving the spasms of pain, but that the taking of each dose was always followed by a distinct throbbing in the head accompanied by some degree of pain and giddiness. He found that its power of warding off an attack of angina lasted for a much longer period than that of the nitrite of amyl, but in this respect was hardly equal to the nitrite of sodium, although the difference was not great. For example, a single dose (ʒ ij) of the nitro-glycerine solution taken early in the morning enabled him to get on until before dinner without the supervention of the pain; but a single dose of it taken before starting home for dinner never enabled him to accomplish the return journey without being subject to the pain, whereas a single dose of the solution of the nitrite of sodium was generally sufficient under similar circumstances to permit of his comfortably accomplishing the double journey. At his own request I permitted him to at once return to the use of the latter.

Since then, until now (8th February), with the exception of a day or so at a time, when he was trying at my suggestion some other combinations of nitrous acid, and whose effects I may make the subject of a future communication, he has continued to use the nitrite of sodium, and he finds that it has not been necessary, in order to obtain the desired effect, to increase the dose, although two days ago, for the purpose, if possible, of hastening the cure of the angina, I have asked him to double the dose.

Towards the end of January I asked him to ascertain the smallest dose which would prevent or relieve each attack of pain, giving him certain instructions for the exact attainment of that object. He found that neither ten, twenty, nor thirty drops of the solution taken after rising prevented the pain coming on before breakfast, but a teaspoonful did; and that the smaller doses merely dulled but did not completely relieve the angina, once it had commenced. A dose, therefore, of the nitrite of sodium of about two grains is evidently sufficient in his case. And even a much smaller dose of a *pure* preparation of the salt may suffice, for, knowing that the nitrite of soda,

unless carefully manufactured, is a little apt to be mixed with the nitrate, I procured a quantity of it two days ago from the druggist who was supplying my patient; and I confess I was somewhat astonished to find that, although obtained from a London manufacturing firm of the highest eminence, the specimen analysed did not contain more than 33 per cent. of the nitrite, the remainder consisting of the nitrate of sodium. This is a very important fact, for its knowledge should serve to prevent failure in the therapeutical use of the nitrite of sodium from the preparation not being what it is supposed to be. If sufficient care is taken in its manufacture, it is not difficult to obtain the salt in an almost pure form. Another specimen of the nitrite which I procured for myself nearly two years ago contains only a trace of the nitrate. The proportion of nitrous acid is easily estimated by means of a standard solution of permanganate of potassium. Allowing, therefore, for the impurity of the preparation used by my patient, the requisite or efficient minimum dose of the nitrite would seem to be in his case a little less than one grain.

The present condition of the patient is thoroughly satisfactory. He very rarely has any pain during the night, although he often wakens about six in the morning with a desire to urinate. The spasms of pain are still apt to come on in the morning and during the day, but never unless after exertion. Formerly they even occurred when he was sitting quietly by his own fireside. When they do come on, and if he does not check them by having recourse to the nitrite, they are very much less severe than they once were, and are now quite bearable, and never last for more than three or four minutes instead of from five to fifteen minutes. His pulse is full, strong, and regular; the cardiac sounds are unaltered. The urine has never exhibited any trace of albumen or glocuse, substances which sometimes appear after the use of nitrite of amyl and nitro-glycerine.

I offer no opinion as to the exact cause of the angina pectoris in this particular case; nor shall I say anything as to the nature of the beneficial action of the nitrite of sodium, beyond remarking that the good effected is evidently accomplished by a very small alteration of the dynamics of the circulation. I may, at another time, discuss the nature of this action.

My principal object has been a therapeutical study of the value of nitrite of sodium, and, incidentally, of some allied substances, in angina pectoris. I greatly regret that the clinical material has been so little; but although only a single case, even had I had many more at my disposal, I would with difficulty have found one better adapted, either by the severity of the pains, or the regularity and frequency of their occurrence,

for the purpose of crucially testing the value of the nitrites. The patient was, moreover, a man of considerable intelligence, which greatly facilitated my experiments. It is quite clear from the results of the treatment of this case that the active remedial part of nitrite of amyl, nitro-glycerine, and nitrite of sodium is the nitrous acid. The suggestion of this is not now new, nor is there any novelty in its practical application, for, based on the analogy of the physiological action of nitrite of potassium to that of nitrite of amyl, Weir Mitchell, of America, was the first to recommend and try the use of a simple alkaline nitrite in the treatment of epilepsy, and later, in this country, Law, Saundby and Ralfe, the two former in this Journal, have recorded cases in which they have employed it with varying success.

But as the pre-eminent therapeutical action of the nitrite of amyl, for the knowledge of which we owe a great debt of gratitude to Lauder Brunton, rests with its use in the treatment of angina pectoris, it is particularly in this disease, against which all other remedies seem to be powerless, that we would expect to obtain the most valuable field for the employment of some simple combination of nitrous acid. And it is extremely satisfactory to find from the case related, which, to my knowledge, is the first case of angina pectoris in which a simple nitrite has been employed, that in the nitrite of sodium, the simplest of all the compounds of nitrous acid, we have a remedy as active in kind and as reliable in effect as nitrite of amyl or nitro-glycerine, and possessing several distinct advantages over either of these in respect, among other things, of producing in therapeutic doses no disagreeable general effects—headache, giddiness, and even partial collapse. Whether the nitrite of sodium is the combination of nitrous acid most to be preferred on pharmaceutical grounds is yet to be determined. The nitrite of potassium would act equally well, but it is rather more deliquescent than the sodium compound. The taste of the latter somewhat resembles that of common salt. The principal objection to the use of the nitrite—but, fortunately, one of little moment when we consider the nature of the disease which it is used to alleviate—will probably be found in the eructations of nitrous acid gas which it is apt to provoke, especially if it be necessary to give it in large doses, but from which the patient in the present case never suffered, owing to the small dose administered. The nitrites, on account of their remarkable physiological action, will, I venture to anticipate, in the form of their simple and more safely administrable compounds, soon have a wide and important application in the treatment of various forms of disease.—*Practitioner, March*, 1883, *p.* 179.

19.—ON THE TREATMENT OF MITRAL VALVE DISEASE.

By A. Ernest Sansom, M.D., Physician to London Hospital, Senior Physician to the N. E. Hospital for Children.

Let us suppose that, owing to rheumatic endocarditis, the mitral valve has been rendered incompetent. It is well known that such may be the case, and yet the subject of such incompetence present no sign nor symptom of deviation from health. We are familiar with cases manifesting the murmur of mitral regurgitation in childhood, who pass through the period of adult-life without suffering from the distresses of cardiac disease, and who, perhaps, ultimately succumb to an affection the course of which the valvular imperfection has in no wise sensibly modified. In such cases, the valvular imperfection has become *compensated*. Supposing a regurgitation just instituted, the first effect is upon the left auricle, which is now made to contain a quantity of blood greater than normal by so much as gushes into the auricular cavity at each systole. The effect is to distend and to dilate the auricle. The left ventricle, too, is filled more rapidly than under normal conditions, because the blood from the auricle enters it under pressure the moment that diastolic relaxation permits. Such entrance of blood is more free than the normal. Hence, dilatation or hypertrophy of the left ventricle, or a *tendency* thereto. The most important of the induced conditions is, however, that of the pulmonic circulation. The reflux current overfills not only the auricle but the pulmonary veins and the pulmonic capillaries. Against such resistance comes the force of the right ventricle in systole, which in opposing resistance becomes hypertrophied. The hypertrophy of the right ventricle is essentially conservative, and the increased tension in the pulmonic circulation is an essential condition of compensation. The sign of such heightened tension and therefore of compensation is, as long ago pointed out by Skoda, accentuation of the pulmonic second sound in the second left interspace.

Observation of the degree of pronunciation of the pulmonary second sound is of the highest importance as regards the treatment of mitral regurgitation. It is in a considerable degree a measure of the amount of such regurgitation. If the aperture caused by incomplete mitral closure in systole be small, the pulmonic tension is only slightly increased, and the pulmonic second sound may not be perceptibly intensified; but, if the gap be wide, the tension, supposing the two ventricles to be in an efficient condition of compensatory hypertrophy, is great in the pulmonic circuit, and the second sound in the pulmonic area is very loud. If afterwards the loudness of such second sound is found to diminish, such sign is of

very high importance. It suggests that the compensatory hypertrophy of the right ventricle is beginning to fail, that dilatation is in excess, and that the tension in the pulmonary artery is reduced by so much as regurgitates through the tricuspid orifice. Of course, the other signs of tricuspid regurgitation should be taken in conjunction with this; but I know of no sign which is so valuable a guide for treatment.

As the left auricle is over-filled in proportion to the amount of blood regurgitating, so is the aorta, and from it the systemic arteries, ill-supplied. A diminution occurs in the normal quantity of blood propelled to the tissues; in the veins circulation is retarded, and the normal content augmented. There are arterial anæmia and venous plethora. The institution, however, of compensatory hypertrophy of the right ventricle rectifies the ill-supply to the aorta. The increased pressure in the pulmonic circuit at the time of systole opposes the reflux into the auricle, and the current thus opposed is urged in normal amount into the aorta. So, even supposing that the force of the left ventricle be not augmented, increased force of the right may restore the equilibrium by inducing a pressure in the auricle equivalent to that afforded by a competent valve.

As regards the mode of production of compensatory hypertrophy, I would draw attention to an excellent chapter in Dr. Milner Fothergill's work. The practical question when a patient comes before us who presents signs of mitral regurgitation, the legacy of rheumatic endocarditis, is this: Is the valvular imperfection duly compensated or not? Subjective symptoms may tell us of such want of compensation, but they are often deceptive. In addition to the auscultatory sign I have mentioned, we may get valuable evidence from the use of the sphygmograph and cardiograph. The former may tell us of a fairly normal tension in the systemic arteries, or otherwise; the latter, by recording the duration of systole and diastole, may tell us how far the normal rapidity of filling of the ventricle is exceeded, and this may give evidence of the amount of regurgitation.

Supposing that we are satisfied that there is due compensation, medicinal treatment may be entirely unnecessary. I have no doubt that a vast amount of injury has been done to patients by a shaking of the head of the auscultator over the subject of a mitral murmur, who, perhaps, was no worse at the time of examination than he was ten, twenty, or thirty years before, and who might continue uninfluenced for harm by the cardiac complication all his days. He should be cautioned against strain, against exposure, and against irregularities. He may be better occasionally for treatment by iron-tonics, cod-liver oil, or strychnine; but any special *cardiac* treatment is out of place.

Not so, however, if there is evidence that compensation is beginning to fail. I will pass in brief review the chief agents which are of service in such case:—

1. *Digitalis* is *facile princeps* of drugs in the treatment of imperfect compensation. The researches of modern observers —Fuller, Handfield Jones, Ringer, Balthazar Foster, Franks, Wood, Lauder Brunton, Milner Fothergill, and others—have shown its mode of action: that it so influences the cardiac ganglia as to induce a more perfect contraction of the ventricular muscle, and hence a more complete emptying of the ventricles; whilst at the same time, by an action of the vasomotor centre, it causes contraction of the arterioles and a heightened tension in the arterial system. It slows the heart by lengthening the diastolic pause. So not only does it give rest to the wearied cardiac muscle, but—as this muscle is nourished only during such diastolic pause by the blood which then enters through the coronary arteries—it directly ministers to its nutrition. It is a matter of common experience that digitalis, especially when combined with iron, strychnine, cod-liver oil, and other tonics, restores the *status quo ut ante* when in a patient manifesting a mitral systolic murmur the evidence indicates that compensation is beginning to fail. As, however, with every other medicinal agent, caution must be used in the administration. As regards *dosage*, a certain golden mean has to be observed. A little over a suitable dose may induce nausea, vomiting, irregularity of pulse, and, instead of slowing, an enhanced rapidity of heart's action; whilst a dose which produces a favourable result is constant and discoverable in regard to a large majority of patients, in a minority such dose is inconstant, and even unattainable.

As regards the preparation used, we may have differences of result, and we know that, as in the case of so many vegetable preparations, the energy of different samples may vary. The pharmacopœial equivalents of the officinal drugs P.B. are a little awkward: One grain of the dried and powdered leaves = one-third of an ounce of the infusion = eight minims of the tincture. Practically I consider the tincture most reliable, and that usually in small doses (♏v. to ♏x., increased only in exceptional cases, and then occasionally reduced); next in value I consider the powdered leaves (gr. ½ ad gr. ij.).

In some cases, even by increasing the dose, no apparent influence appears to be exerted by the drug. Then digitaline, especially when hypodermically injected, I have observed to give in many cases good results. Digitaline hitherto prepared has probably scarcely ever been the pure alkaloid, but it appears likely that by a new process it can be obtained in a state of purity. The dose for hypodermic administration is 1-50th of a grain. In a

child of ten years of age suffering with dropsy and great distress from mitral regurgitation I have found after injection of 1-100th of a grain of digitaline hypodermically, administered at intervals of four hours, the pulse-rate reduced by 8 per minute after each injection. In this case recovery took place from the urgent symptoms, and the child was sent to a convalescent home. She relapsed, however, and died three months afterwards, when away from our observation.

When the right ventricle has dilated so far that there is marked tricuspid regurgitation the beneficial action of digitalis is by no means so decided. Nevertheless, in some cases, especially when occasional purgation is a part of the plan of treatment, the signs of tricuspid regurgitation may pass away. For instance, in a child (Alice B.), aged 11, under my care at the North-Eastern Hospital for Children, mitral regurgitation with dropsy was manifested, and marked venous pulse was seen in the left external jugular. Treatment consisted of 6-minim doses of tincture of digitalis three times a day. The child had taken previously as out-patient 4-minim doses, with 4 grains of ammonio-citrate of iron, three times a day. After 21 days all severe symptoms had passed away, there was no venous pulse, and the case was discharged as convalescent two days afterwards.

In other cases no such favourable result attends. In fact, as *à priori* considerations might suggest increased force of systole which the digitalis may bring about serves to urge back the blood through the imperfect tricuspid orifice into the venous channels. But then, I have seen good results when the administration of digitalis has been combined with abstraction of blood by leeches or cupping. In a child of 10 (Maria W.), manifesting mitral and tricuspid murmurs with percussion, evidence of greatly dilated right ventricle, after rest in hospital for a fortnight and administration of tincture of digitalis in 4-minim doses, with tincture of the perchloride of iron (♏x.), and a single leech applied to the epigastrium every other day for 14 days, it was noted that the dulness over the right cavities receded to the mid-sternal line coincidently with general signs of amendment. I prefer very small abstractions of blood repeated every two or three days to larger bleedings at longer intervals. In a case lately under my care at the London Hospital this lesson seemed to be pointed, though the recovery was very satisfactory :—

Alice F., aged 11, was under my care for mitral and tricuspid regurgitation, with great and advancing œdema, orthopnœa, and cardiac distress. She was treated by 20 minim doses of tincture of perchloride of iron with 5 minims of tincture of digitalis. After twenty days tincture of casca was substituted

for digitalis, with no apparent benefit. Digitalis was then resumed as before, and considering the great distension of the right chambers, 6 leeches were applied to the chest. Great relief of dyspnœa followed, and œdema became less. Improvement was maintained for ten days, and then urgent dyspnœa and signs of greater dilatation of right chambers occurred. Casca was again tried, and this time with some apparent benefit. Purgatives (pulv. jalapæ co., ʒ ss.) were also administered, but still these grave signs of right ventricle engorgement continued. Again, 6 leeches were applied to the præcordia. A few days afterwards a great vein trunk in the right upper arm was found to be plugged, and the whole arm and forearm enormously swollen. It seemed to me possible that the abstraction of blood, by rendering coagulation more easy, had disposed to the thrombosis. Nevertheless, I was convinced that the cardiac trouble was sensibly relieved by the leeching, and this was repeated, and saline diuretics and digitalis again administered, in doses increasing from ℥v. (vij., ix., x., and xx.) Under this treatment there was gradual, but very marked improvement. After five days of the full dose of digitalis it was altogether omitted for ten days, and then resumed in ℥x. doses. All the urgent symptoms passed away, the enormous swelling of the arm due to the venous thrombosis entirely subsided, and the patient was discharged convalescent and able to walk with comfort, after having been in hospital for six months.

2. *Belladonna* is, I think, only useful in the treatment of failure of compensation in cases of mitral regurgitation when combined with, or *occasionally* substituted for, digitalis. Belladonna, like digitalis, increases the power of systole, and raises the arterial tension. As Dr. Lauder Brunton has shown, it paralyses the cardiac terminals of the vagus, and reduces irritability by an anæsthetic effect on the sensory nerves of the heart. Very useful occasionally, it by no means compares with digitalis for prolonged employment. The hypodermic employment of digitaline 1-50th gr. with atropia 1-60 gr. I have found very satisfactory.

3. *Casca*, a tincture made from the bark of *Erythrophlœum Guinense*, the ordeal bark of West Africa, has been employed as a substitute for digitalis. Dr. Brunton, in his Gulstonian Lectures for 1877, published the results of his elaborate experiments as to its physiological action. In kind this action appears much to resemble that of digitalis. Dr. Brunton has said "Digitalis has hitherto been our great resort in mitral disease, but I think it probable that in casca we possess a drug more powerful still; at least, its effect upon the arterioles appears to be greater than that of digitalis, and it is quite possible that it may succeed in those cases of advanced mitral

disease where digitalis fails." I have myself employed the tincture of casca substitutively for digitalis in a considerable number of cases, but I have never yet been able to convince myself that it has any more beneficial action in mitral disease.

4. *Caffeine.*—Gubler, Shapter, Leech, Milliken, Brakenridge, Huchard, and others have recorded observations showing the beneficial action of caffeine (or its citrate) in cases of cardiac disease, especially where dropsy is a marked symptom. Some of the cases show very forcibly that a favourable influence has been exerted by the drug. There are many apparently contradictory data as to its physiological action, but the cardinal points are that it first quickens, but soon after slows, the heart's action; that it increases the general arterial tension; and acts in a very pronounced manner as a diuretic in cardiac dropsy. Dr. Brakenridge advises that digitalis be administered previously to, or in conjunction with, the citrate of caffeine, and that small doses (gr. iij.) should be employed. M. Huchard, however, recommends that caffeine, and not its citrate, should be used, and that in larger doses (gr. iv. to gr. v.). It produces diuresis more rapidly than digitalis, and has none of its nauseating effect. I have employed citrate of caffeine in substitution for digitalis, and without any marked benefit being manifest; indeed, I have found that in some cases it has induced insomnia. Nevertheless, I consider that the evidence is such that I should certainly employ it in any case where, in cardiac dropsy, a rapid diuretic effect was desirable.

5. *Convallaria majalis.*—This is the well-known lily of the valley, long employed by the Russian peasantry as a remedy for dropsy. It is, botanically, closely allied to asparagus, the diuretic effect of which is well known. M. Germain Sée has made a series of researches which point to it as probably a valuable agent in the treatment of failure of compensation in cardiac disease. The preparation used is an extract of the whole plant, flowers, stems and roots. The mode of action of the extract of convallaria also resembles that of digitalis—it slows the heart whilst increasing the force of systole, and augments arterial tension. It is said that it does not, like digitalis, exhaust the contractility of the heart and arteries. Administered in doses of 15 to 22½ grains during the day it has apparently produced very favourable results. M. Sée has recorded five cases of mitral regurgitation in which it was employed. It entirely relieved the œdema and cardiac distress, and manifested a pronounced diuretic action. I am now employing the extract of Convallaria in mitral disease in 5 grain doses. I am convinced of its power of raising the intra-vascular pressure, and of its increasing the force of systole, but I am not yet convinced of its superiority to digitalis. The results, however,

are such as to warrant an extended trial. I shall have to speak of it again in regard to the treatment of aortic disease.

6. *Morphia.*—Judiciously employed, I consider that this is one of the most valuable of agents, or rather adjuncts, in the treatment of the distress, especially the dyspnœa and insomnia, attendant upon failure of compensation in cases of mitral regurgitation. I am strongly of opinion that it should not be administered by the mouth, but by hypodermic injection. When given by the mouth, it disagrees, just as opium frequently does, whereas, administered hypodermically, it calms the most distressing dyspnœa without inducing, so far as my experience goes, any ill effect. The value of the hypodermic use of morphia in the distress of heart disease, was brought before the profession, in his usual forcible and able way, by Dr. Clifford Allbutt in 1869. I entirely endorse his view of the value of this remedy and its innocuousness in cardiac failure. I have found it valuable to combine the morphia (usually a hypodermic dose of one-third of a grain) with atropine (gr. 1-60th), or digitaline (1-50 gr.), but the morphia is an essential agent in the successful treatment of mitral regurgitation, when there is much respiratory distress.

Coincidently with such special cardiac treatment, general measures should be adopted for securing improved nutrition. The heart-muscle must not only be preserved from wasting, but it must be fed. The problem of administering a due amount of nourishment is often a difficult one. Dr. F. J. Roberts has recommended in the gastric crisis of cardiac disease, when there is an almost complete inability to take food, the use of peptonised aliment in a sipping fashion. This plan I would combine with the administration of nutritive enemata, as I have before mentioned. I feel sure, from my experience, that lives may be prolonged and crises tided over by such supplementary alimentation.

The foregoing is a brief sketch of the most important agents now at our disposal for restoring the power of the heart-muscle and inducing due compensation in mitral regurgitation, occasioned by rheumatic endocarditis, when failure threatens. The restoration of such compensation may not however, be the only indication. Accidents of the disease, so closely related therewith as to force the necessity of considering them in any question of prognosis and treatment, demand consideration. Such epiphenomena are renewed attacks of endocarditis, pericarditis, especially when accompanied by myocarditis and adhesions and embolism. These subjects, however, being equally manifest in mitral stenosis and mitral regurgitation, may be conveniently postponed till the next lecture.

There is, however, yet another group of cases of mitral

regurgitation to consider. In these, there is no history of antecedent rheumatism, the modes of causation we have hitherto discussed are excluded, the condition has arisen gradually in *association with conditions of high tension in the arterial system.* The differentiation of this class of cases is important, both for prognosis and treatment. A considerable minority of cases manifesting regurgitation come to us with no history whatever of rheumatism. I calculate from the hospital records that these are about one-fourth of all the cases. I have said, however, in my former lecture, that rheumatic endocarditis may be established without other evidence of rheumatism: it is obvious, therefore, that such cases of insidious endocarditis inducing mitral insufficiency may be included in the minority we are considering. The diagnosis between these and the cases of regurgitation due to yielding of the ventricle from intra-arterial pressure may generally be arrived at without difficulty. In the latter, the signs are those of hypertrophy rather than dilatation of the left ventricle. The aorta second sound, if there be no evidence of aortic disease is pronounced whilst (the amount of blood regurgitating being small) the pulmonary second sound is not accentuated. The arterial pulse is strong and incompressible, and the arteries are often tortuous and visibly atheromatous. It is obvious that these signs are very different from those usually met with in the rheumatic cases. Very important evidence is afforded by the sphygmograph, the pulse trace showing in the non-rheumatic cases an ample tidal wave, and the usual evidences of high arterial tension. Such cases are often associated with gout or with chronic renal disease.

In their treatment I have known no plan so successful as a protracted course of alkalies with abstinence from alcohol, and as much rest as can be procured. Digitalis is not contra-indicated, notwithstanding the general high-pressure in the arteries. I have found it of much service, probably by co-ordinating heart and arteries. In any of the accidents of high tension, such as symptoms of angina or dyspnœa occurring in this class of patients, the administration of nitro-glycerine, or the inhalation of nitrite of amyl is of much service.—*Medical Press and Circular, Feb.* 7, 1883, *p.* 111.

20.—ON CONVALLARIA MAJALIS IN HEART DISEASE.

By the Editor of the British Medical Journal.

The common lily of the valley (*Convallaria majalis*) has recently been introduced as a diuretic and remedy for certain forms of heart disease. It has hitherto been more generally valued for its poetical associations, and its delicate beauty and

its fragrance, than for its medicinal properties, but even in ancient times it was used as a curative agent, and was employed in the treatment of wounds, bruises, worms, intermittent fever, and epilepsy. By Culpepper it was regarded as a sovereign remedy for "a weak memory," and he tells us that the spirit of the flowers distilled in wine will restore lost speech. He adds that, "it is exceeding good in apoplexy," and that it comforteth the heart and vital spirit. Gerarde recommends that the flowers should be placed in a glass, and put in an ant-hill, when after a month a liquor will exude, "which being outwardly applied, helps the gout." It is well known that from time immemorial the peasants of Eastern Europe have regarded the lily of the valley as a certain remedy for dropsy. In 1880, two Russian physicians, Troitzky and Bogojavlensky, investigated its action, and published several cases showing its value in the treatment of various forms of cardiac disease. These statements were for the most part confirmed by Prof. Botkin of St. Petersburg. Still more recently, the subject has been systematically studied by Prof. Germain Sée at the Hotel Dieu, Paris, and the results of his experiments and observations are given in the *Bulletin Général de Thérapeutique*, July 30th, 1882.

The active principle of convallaria has long been known. In 1858, Walz succeeded in isolating two glucosides, which he named "convallarin" and "convallamarin." Their chemical properties were investigated by Tânret and others, whilst Marmé published a paper on their physiological action. It was found that convallarin possesses purgative properties only, whilst convallamarin is a heart poison allied to digitalin, helleborin and the upas principles. Stanislas Martin is supposed to have discovered in the fresh flowers an alkaloid, which he named "maialine," but other investigators have failed to detect it in the leaves, stem, or root. The preparations commonly employed are the aqueous extract of the leaves, the aqueous extract of the flowers, and the extract of the whole plant. A watery infusion of the leaves and flowers is sometimes recommended, but to obtain the full therapeutic effect it is better to use an extract of the whole plant. A reliable preparation is made by Parke, Davis and Co., of Detroit.

A drop of the extract of the flowers, injected under the skin or applied topically to the frog's heart, arrests it in systole, in much the same way as digitalis, upas antiar, erythrophleum, and some other remedies. In the case of a dog, four drops of this extract, injected into a vein, caused death in ten minutes by arresting the heart's action. It appears that the heart is first slowed, and the respirations are quickened; then the heart's action becomes irregular, and the pulsations are weak and very rapid; the blood-pressure is first increased, and is then lowered;

the respirations become slower and slower; the heart ceases to beat; the pressure falls to zero, and the respiratory movements cease. The excito-motor power of the nerves is unaffected, and the excitability of the pneumogastric is weakened, although not abolished.

Dr. Sée has published details of twenty cases, in which the effects of convallaria were carefully noted. Five were cases of mitral insufficiency, characterised by want of rhythm, œdema of the lower extremities, dyspnœa, and other symptoms. The extract was given in doses of from half a gramme to a gramme daily, and in each case the benefit was most marked; the heart's action became stronger and more regular, the breathing improved, and there was a notable increase in the amount of urine passed. The sixth case was one of mitral constriction, which was immediately benefited, there being a speedy improvement in the pulse, with increase of urine and decrease of the œdema. Several cases of aortic insufficiency were relieved of the more distressing symptoms.

Dr. Sée considers that Convallaria majalis constitutes one of our most important cardiac remedies. It produces on the heart, blood-vessels, and respiratory organs, effects constant and constantly favourable; to wit, slowing of the beatings of the heart, with often a restoration of the normal rhythm; and, on the other hand, augmentation of the energy of the heart, and of the blood-pressure. Dr. E. P. Hurd has published a remarkable case of Corrigan's disease, in which the fluid extract prepared by Parke, Davis, and Co., proved most beneficial, given in five drop doses every four hours.

The therapeutical indications for the employment of convallaria are beginning to be generally recognised. It is useful in palpitation resulting from exhaustion of the pneumogastrics; in simple cardiac arythmia, with or without hypertrophy, and with or without valvular lesion; and in mitral constriction, with absence of compensation in the left auricle and right ventricle. It may be given with advantage in mitral insufficiency, especially when there is pulmonary congestion with resulting dyspnœa. In Corrigan's disease, the peripheral arterial pulsations disappear, and respiration becomes markedly restored. In dilatation of the heart, with or without hypertrophy, convallaria is decidedly indicated. In cardiac dyspnœa, it is inferior to morphia or iodide of potassium; but, in some forms of asthma, it is said to be useful.

Convallaria exerts no deleterious effects, and may be given with perfect safety, Dr. D'Ary says: "With me, it has long since taken the place of digitalis; and in cases of organic heart-disease, when, in the latter stages, the symptoms are becoming urgent, and the patient anxiously begs for relief, the

physician will appreciate a remedy that will relieve signally and promptly, without the dread of overdose and cumulative action." At the present moment, convallaria is difficult to obtain; but in a few months there will be an ample supply.—*British Medical Journal, Feb.* 24, 1883, *p.* 368.

21.—ON THE USE OF CONCENTRATED SOLUTIONS OF SALINE CATHARTICS IN DROPSY.

By MATTHEW HAY, M.D., Demonstrator of Practical Materia Medica in the University of Edinburgh.

In the course of an investigation of the physiological action of saline cathartics, now in course of publication in the Journal of Anatomy and Physiology, I examined the effect of the administration of a saline cathartic on the concentration of the blood, and I succeeded in demonstrating from experiments on man and the dog that, if the salt be given in the form of a concentrated solution when the alimentary canal of the animal contains little or no fluid, it produces an almost immediate and very decided concentration of the blood, owing to the blood becoming deprived of a large amount of its water through the intestinal secretion which the salt excites; if, however, the salt be given dissolved in sufficient water, or if the alimentary canal contain sufficient fluid at the time of the administration of the salt, no such concentration of the blood occurs. In the former case the hæmatic concentration is very considerable, and is very quickly produced. It reaches its maximum within half an hour after the ingestion of the salt; and is so marked that after giving three-quarters of an ounce of sulphate of soda dissolved in three ounces of water to a man, whose alimentary canal must have been practically free from fluid, I found that the number of blood corpuscles in each cubic millimetre of his blood rose from about 5,000,000 to 6,790,000. An almost similar result was obtained with a dog. This excessive state of concentration does not last very long. In from one to one and a half hours after the administration of the salt it begins to decline, and continues to do so until at the end of about four hours the concentration is reduced to the normal. This reduction is effected not by the absorption of fluid from the intestines, but by the abstraction of lymph and other fluids from the tissues, and the quantity thus abstracted must be very large. These alterations of the volume of the blood take place apparently without any corresponding change of the blood-pressure. The blood, therefore, would appear to abstract the tissue fluids in virtue solely of its concentrated condition, and, in the nature of its action, to behave somewhat like a sponge.

The presence of the salt in the blood may also influence the tissue fluids by acting on them endosmotically. Some hours after the administration either of a concentrated or of a dilute saline solution the blood undergoes another concentration, less in degree than the first, but continuing for the greater part of the day. This second concentration is evidently due to the diuretic effect of the absorbed salt. Doubtless, during this period also, as during the first concentration, the tissue fluids are being drawn upon.

From these remarks it will be readily understood how that a concentrated solution of a saline cathartic ought to prove of considerable service in certain cases of dropsy, where, owing to the great accumulation of transuded serum in vital parts and elsewhere, there is imminent danger to life and an urgent need for an immediate and active removal of a portion of the transuded fluid. In such cases the value of saline and other active cathartics has long been appreciated, but I am not aware that use has been made of the more powerful action of a concentrated saline cathartic. It removes the dropsical fluid by two channels: by the intestines and by the kidneys. No other purgative has this double action. It is questionable, also, if any other purgative acts so rapidly in reducing the fluids of the blood. This is of the greatest importance in certain critical cases of dropsy. It is almost perfectly certain that no other purgative excites intestinal secretion so powerfully, and at the same time produces so little irritation of the intestinal mucous membrane and so little disturbance of the body generally. This is an additional recommendation for the employment of the concentrated saline. The diluted salt, the form in which it is always given, has practically, in so far as it affects dropsical fluids, the action only of a diuretic. Based on these considerations, I have made several trials of the concentrated salt in suitable cases of dropsy, and in most of them with very satisfactory results. It is sufficient for my present purpose to give details of one of these cases.

J. A——, a young lad, aged ten, had been an out-patient of the Edinburgh New Town Dispensary for over a year, when, as one of the physicians to the dispensary, I was asked to visit him. He had been suffering for several years from heart disease. I found him propped up in bed, being unable to lie down, and showing signs of great distress. His breathing was rapid and shallow, and he was suffering from great dyspnœa, with frequent coughing. There was a well-marked ascitic distension of the abdomen and general anasarca, observable especially in the lower limbs. A loud mitral regurgitant murmur could be distinctly heard over the whole of the front of the chest. The pulse was rapid, small, and weak. An abund-

ance of soft râles, audible all over the chest, especially towards the base of the lungs, indicated a pronounced œdema of these organs. The dyspnœa was so great that he sat with his arms straight and almost perpendicular, and with the hands pressed on the bed, elevating and fixing the shoulders to bring the extraordinary muscles of respiration into play. He had been treated previous to my seeing him with almost every variety of renal and cardiac stimulant, and at intervals with cathartics. At the time I visited him, besides some medicine to relieve the cough, he was having iron and digitalis several times a day. In spite of this active medicinal treatment, his condition was gradually becoming more serious, and the dropsy and breathlessness were increasing. The administration of a saline cathartic, dissolved and diluted in the usual way, had been tried, it so happened, two days before I saw him, but its action was attended with only slight relief. As I thought this a suitable case for the use of the concentrated saline, I ordered that he should have as little as possible of food and liquids during the night, in order to free the alimentary canal from digestive juices or other fluids, and permit of the full action of the salt, and I asked his mother, who was nursing him, to give him next morning three-quarters of an ounce of sulphate of magnesia dissolved in two tablespoonfuls of water, no water to be given afterwards. The result exceeded my expectation. When I called next evening, the patient was lying quietly sleeping in his bed. The anasarca was greatly diminished, and the dyspnœa had almost entirely gone, and his breathing was much slower. The pulse was also less rapid, and the pained, anxious expression of his face had vanished. His mother told me that she had given him the salt as I had directed, and that in less than an hour afterwards the purgative action of the salt manifested itself, and there were repeated evacuations in the course of the next few hours; on each occasion the water seemed to "gush" from him, and he passed an unusually large quantity of urine. There evidently had not been merely a removal of so much fluid from the blood and tissues as was necessary for the usual dilution of the salt within the intestines, but the sharp, sudden withdrawal of fluid from the tissues by the concentrated blood had initiated a movement of the fluid into the latter, which had continued for some hours after the direct action of the salt and the blood had ceased, and until the tissues were in great part rid of their superfluous liquid. Next day I was pleased, on entering, to find him on the floor amusing himself with the other children, and looking perfectly comfortable and happy, and, except for his wasted and pallid features, showing little evidence of having been quite recently so dangerously ill. I was told

he had not felt so well for five months before. I continued to see him for a month afterwards, during which time he resumed taking the digitalis and iron, and throughout this period he had no return of the dyspnœa, and the dropsy was comparatively trifling. Owing to the removal of the family afterwards to England, I obtained no account of his further progress. The benefit of the concentrated purgative was in this case very striking, and, perhaps, more so than in any other case in which I used it. The conditions necessary for the successful administration of the salt are that the nature of the dropsy should be such as to permit of the full action of the salt. I have found it more useful in general dropsies than in local dropsies, and of general dropsies more beneficial in those dependent on a stasis of the circulation, as cardiac dropsy. The cases in which I have employed it are, however, as yet too few to warrant me in making definite generalisations. The other requisite conditions are that the alimentary canal, by the previous abstinence of the patient for some hours previously from food, and especially liquids, should be allowed to become as free from fluid as possible, and that the salt should be administered along with the smallest possible quantity of water. Sulphate of magnesia, on account of its being soluble in less than its own weight of water, is one of the most suitable of the saline cathartics for this purpose. Sulphate of soda is, owing to its greater insolubility in water (1 in 4), less suitable. The alkaline tartrates and Rochelle salt do not, however, present this objection, and may therefore be found useful. The phosphate of soda and the sulphate of potash are too insoluble to be of any service.—*Lancet, April* 21, 1883, *p.* 678.

DISEASES OF THE ORGANS OF RESPIRATION.

22.—ON TUBERCLE.

By J. Burdon Sanderson, M.D., F.R.S., &c.

From a summary of the views entertained by the principal pathologists who have directed their attention to the subject, it will be seen how numerous are the aspects in which the results of tubercle inoculation may be regarded, and how complicated are the questions which arise out of them. It is important, however, to notice that in so great a diversity of opinion there are certain particulars in respect of which all are agreed. Thus it is generally admitted that the certain result of the inoculation of tuberculous matter of whatever kind is to produce crops of miliary granulations in the lungs and other internal organs; that these granulations are anatomically

identical with grey tubercle, that they become yellow and opaque, and eventually break down into a soft cheesy material. It is further admitted on all hands that the process is, so to speak, a self-diffusive one, that in the act of inoculation a new element is introduced into the inoculated animal at the point of insertion, from which it is disseminated to all parts of the body through the lymphatic and arterial circulation.

At this point agreement ceases. It is neither agreed what the new element is, nor what is the nature of its effect on the blood, or on the living tissues to which the blood-stream carries it. With reference to these questions we find three views indicated. They may be expressed as follows: (1.) The material introduced consists of microscopical particles, possessed of no properties excepting those they owe to their hardness, form, and magnitude, and therefore capable of producing no effects excepting those of mechanical irritation. (2.) The material consists of living matter, capable either of growth by assimilation of new matter similar to itself, or of causing morbid transformations in the tissues with which it comes into contact; in either case acting in a specific way which cannot be referred to its physical properties. (3.) It is a definite chemical compound which is soluble in the blood plasma, and has a special and exclusive action on certain tissues, particularly that of the lymphatic glands and other analogous structures.

Practically, the first and second of these propositions are the only important ones; for, although Prof. Lebert has suggested the co-operation of a chemical with a mechanical action, he has given no explanation of his meaning. That it is possible to conceive of such an agent as is described in the third proposition can scarcely be disputed, but the extremely small quantity of the material used, taken together with the postponement and protracted duration of the results, are facts of which no intelligible chemical explanation can be given, so that the tissue may be narrowed, so far as relates to the origin of the morbid process, within the limits indicated in propositions 1 and 2. If, on the one hand, the lesions can be fully accounted for by the entrance of minute particles of hard, but absolutely insoluble and innocuous material into the arterial circulation, we are bound to accept this explanation as the simplest, and to admit with Prof. Lebert that the granulations are merely circumscribed inflammations. If, on the other hand, none of the effects can be explained on this mechanical principle, little seems to be left to us but to admit that the contagium of tubercle acts in the same way as the contagia of the so-called specific diseases. Lastly, if the distribution of the granulations and their similarity to those produced mechanically

should seem to suggest that their mode of origin may be mechanical, while other influences manifest themselves in their development, we are perfectly at liberty to adopt the mechanical explanation, so far as it goes, and to supplement it with such other explanations as the facts may warrant.

As regards the bearing of the facts of inoculation on the general question of the nature and origin of tuberculosis, I concluded from my own observations that there is no structural distinction between the artificial disease and human tubercle, so long as the term is confined, as all accurate writers are now accustomed to confine it, to miliary tuberculosis; but I considered it necessary to maintain a reserve as to its relation with the many pathological processes which are spoken of as tuberculosis in the common language of practical medicine and surgery. In going so far the two distinguished pathologists already quoted have fully agreed with me. Dr. Fox says: "I must confess that sceptical as every one must naturally at first feel on this subject, the cumulative force of the evidence in favour of the tubercular nature of these growths appears to me irresistible. We are either dealing with tubercle, or we have before us a new and hitherto unknown constitutional disease of the rodentia, consisting of growths which, in their naked-eye appearances and histological characters, correspond with all the essential features of tubercle in man; which occur not only in the organs which are the chosen seats of tubercle in man, but also in the same parts of those organs; which have the same vital characters and the same early degenerative cheesy changes, not suppuration nor acute softening, and with no marked characters sufficient to distinguish them from tubercle." Cohnheim says: "All the marks by which tubercle is characterised, as such, are present; the agreement of the product of inoculation with human miliary tubercle could not be more complete than it is, whether regard be had to its extended distribution, and to the great variety of organs affected (peritonæum, pleura, lungs, liver, spleen, lymphatic gland, and even the choroid), or to its macroscopical and microscopical characters."

Conclusions.—1. The characteristic product of tuberculosis is not an aggregation of shrivelled particles of irregular form, but a tissue composed of lymph corpuscles held together by a network of hyaline connective substance.

2. There is a close structural analogy between this tissue and that of certain follicular organs belonging to the lymphatic system, *e. g.* the follicles of Peyer, the ampullæ of the lymphatic glands, &c.

3. All the favourite seats of tubercle are naturally characterised by the presence of this tissue, which, from the analogy stated above, may properly be called *adenoid*.

4. The natural distribution of adenoid tissue in the body is in intimate relation with the lymphatic system. In the great serous membranes (which v. Recklinghausen's discoveries have taught us to regard as walls of lymphatic reservoirs) it forms sheaths round the blood-vessels, or masses of microscopical dimensions and irregular contour underneath the epithelium. In the solid viscera it is distributed here and there in the course of the lymphatic channels.

5. In the peritonæum, tuberculosis primarily consists in the enlargement or overgrowth of these sheaths or microscopical masses of adenoid tissue, and consequently the tuberculous nodules which are formed have the same intimate structure, and stand in the same anatomical relation to the vessels and epithelium. In the viscera the essential lesions also consist, not in new growth, but in overgrowth of pre-existing masses of adenoid tissue.

6. The primary local lesion in artificial tuberculosis, whether the cause be simple wound or specific inoculation, consists in the development at the seat of injury of granulations or nodules which have similar structural characters with those of adenoid tissue elsewhere, but cannot as yet be shown to be in relation with the absorbent vessels.

7. The first step in the dissemination of tubercle consists in its being absorbed primarily by the lymphatics (which convey it to the lymphatic glands of which they are tributaries), and secondarily by the veins. Having thus entered the systemic circulation, it is distributed universally by the arteries. The serous membranes seem, however, by preference to appropriate it, and from them it extends by contiguity to the superficial parts of the organs which they cover.

8. The final stage of the process consists in the tertiary infection of the glands of each diseased organ, which glands consequently undergo enlargement and induration, and eventually become partially caseous. The enlargement is due to the multiplication of cells in all the tissues of the organ, but more particularly in the alveoli—the hardening to a process of fibrous degeneration — while the caseation consists in slow necrosis of the previously hardened and anæmic parts. From the first the gland is incapable of performing its functions, but it is not until induration commences that the absorbents of the organ to which it belongs are completely obstructed.

9. In the liver of the guinea-pig, and in some other organs, tuberculous tissue undergoes a fibroid degeneration and caseation, the results of which cannot be distinguished from those observed in the normal adenoid tissue of the lymphatic glands and of the spleen.

10. As regards the question of a *specific contagium* of tubercle,

we think it very important to note that this is not as yet disproved by the facts of traumatic tuberculosis. It still remains open to inquiry whether or not injuries which are of such a nature that air is completely excluded from contact with the injured part are capable of originating a tuberculous process. The results of the following experiments undertaken at the instance of Mr. Simon, with special reference to this question, seem indeed to suggest that they may not be so. Setons steeped in carbolic acid were inserted in ten guinea-pigs on the 24th of September, 1868, each animal receiving two. At the same time extensive fractures of both scapulæ were produced on five others, care being taken not to injure the integuments. No tuberculosis or other disease of internal organs resulted in either of the cases. The facts certainly point to the necessity of further investigation in this direction.—*Practitioner, Dec.*, 1882, *p.* 418.

23.—ABSTRACT OF A REPORT ON THE RELATION OF MICRO-ORGANISMS TO TUBERCULOSIS.

By W. Watson Cheyne, M.B., F.R.C.S., King's College Hosp.

A visit was paid by the author to Prof. Toussaint of Toulouse, and to Dr. Koch at Berlin, with a view of seeing their methods of experimentation, and the results which they obtained. Various experiments were seen, and a quantity of material was brought back to England for more detailed examination. The result of the visits, and a full account of the observations made, will be found in the complete Report presented to the Association for the Advancement of Medicine by Research in the April number of the Practitioner.

It was thought advisable in the first instance to repeat some of the experiments which have led observers, more especially in this country, to object to the view of the specific origin of tuberculosis, and to hold that in rodents, at least, any irritation might produce that disease. The present series of experiments were performed under the best hygienic conditions, with complete isolation of the animals from each other, and with thorough disinfection of the instruments employed. In six cases setons of various kinds were introduced both subcutaneously and into the anterior chamber of the eye; in ten, vaccine lymph both from the calf and from man was employed; in three pyæmic pus was injected (1) into the eye, (2) subcutaneously, and (3) into the abdominal cavity; and in six various materials (cork, tubercle hardened in alcohol, and worsted thread) were introduced into the abdominal cavity. None of these twenty-five animals became tuberculous. Some experiments are also cited in the report in which wounds in rodents have been

stitched up with cotton thread, and others in which abscesses have been produced in various ways, but in none of these cases did tuberculosis ensue. In explanation of the former results, it is pointed out that at the time the early experiments on this subject were made the communicability of tubercle by mediate contagion was not recognised, and, as the precautions necessary for thorough disinfection of instruments, &c., had at that time not been made out, the channels for the introduction of specific micro-organisms were left unguarded.

Two tubes of serum containing micrococci were obtained from M. Toussaint, who holds that micrococci are the cause of the disease. Toussaint obtains the organisms by inoculation of flasks containing serum, or infusion of rabbit with the blood of tuberculous animals; and he has in some cases succeeded in producing tuberculosis by the injection of these cultivations into other animals. The material obtained from M. Toussaint was injected into three rabbits, two guinea-pigs, one cat, and one mouse, and of these seven animals six were under observation for a sufficient length of time for the development at least of local tuberculosis. In no instance did tuberculosis ensue. (In all the experiments detailed in this report inoculation was made into the anterior chamber of the eye, whenever this was practicable; syringes purified by heat were employed for the purpose.) Cultivations of these micrococci were also made, and injected into nine rabbits and three guinea-pigs. Of these four rabbits and three guinea-pigs were under observation for a considerable time without the development of tuberculosis in any case. The total result is that thirteen animals were inoculated with the micrococci with which Toussaint works, and obtained from Toussaint himself, and in no case did tuberculosis occur.

A number of tuberculous organs from animals experimented on by M. Toussaint were also obtained, some of the animals having become tuberculous after the injection of the micrococcal fluid. Careful examination of these organs has shown the presence, often in large numbers, of the tubercle bacillus described by Koch, but no micrococci have been found. The conclusion arrived at is that the micrococci described by Prof. Toussaint are not the cause of tuberculosis. One of the possible explanations of the results which should not be left out of account is the following:—Professor Toussaint trusts greatly to carbolic acid as a disinfecting agent for the purification of the instruments employed in inoculation. This antiseptic, though effectual for the destruction of the ordinary forms of micro-organisms, as evidenced by the satisfactory results obtained from its use in antiseptic surgery, has been shown to be ineffectual against the spores of bacilli, unless it acts for a

long time. The bacillus of tubercle apparently produces spores, and there is no reason to suppose that these are less resistant than those of bacillus anthracis, and other bacilli. An experiment is given which shows that a saturated watery solution of carbolic acid, even though it acts as long as fifteen minutes, is not sufficient to arrest the development of the tubercle bacilli. Therefore, to wash a syringe with carbolic acid is not such a certain means of disinfection in this particular instance as was formerly supposed. Experiments were also made with cultivations of tubercle bacilli obtained from Dr. Koch. Twelve animals were inoculated with these organisms, chiefly into the anterior chamber of the eye, and all of them became tuberculous, and that more rapidly than after inoculation of tuberculous material. The tubercles produced in these cases were infective, and caused tuberculosis in other animals. On examination of tuberculous material, Koch's tubercle bacilli are always found, though in varying numbers. They are most numerous in bovine tuberculosis, and least numerous in human tuberculosis. About eighty organs of tuberculous animals and thirty-six cases of human tuberculosis were examined, and in all of these, without exception, tubercle bacilli were found. The inoculation of these bacilli is more certain and more rapid in its effect than the inoculation of tuberculous material from any source; and this seems only explicable on the supposition that in the cultivation of these bacilli the virus of the disease is present in a more or less pure state, and in large amount. Various facts are pointed out leading to the conclusion that in these bacilli we have the virus of the acute tuberculosis caused in the lower animals by the inoculation of tuberculous material.

In applying the facts obtained from experiments on animals to the pathology of tuberculous diseases in man, it is pointed out that all that has as yet been absolutely proved is that a variety of materials in man, which we class together as tuberculous, produce acute tuberculosis when inoculated into rabbits, guinea-pigs, and other animals, and that this result is due only to the tubercle bacilli present in the materials inoculated. It therefore remains for inquiry, what relation these bacilli bear to the morbid processes in man in which they are found.

Acute miliary tuberculosis in man resembles in every respect, in histological structure, in tendencies, and in the presence of bacilli, the disease produced in the lower animals by the inoculation of tuberculous material; and there can be little doubt that the cause of both diseases is the same—viz., the tubercle bacillus. It is, however, much more difficult to understand the relation of these organisms to localised tuberculous processes in man (phthisis, scrofulous diseases of glands, joints, &c.). Phthisis is alone considered in the present report, and,

with a view of making clear the conception which the author has formed as to the relation of bacilli to this disease, the following facts are brought forward, which he observed as to the mode of distribution of the organisms in the tissue, and their relation to its histological elements.

Two distinct structures have been described as tubercles in lungs of rodents—viz., nodules of lymphatic tissue in close proximity to the vessels and bronchi, and nodules which are largely made up of epithelioid cells. If a case of commencing artificial tuberculosis be examined, it will be found that bacilli are only present in the latter nodules; indeed, it is rare even in the later stages to find them in the former, and in that case epithelioid cells will be found as well. The bacillus being the cause of this disease, the nodules containing epithelioid cells are alone tubercles. Further, on careful investigation of these nodules, it will be found that bacilli are only present in the epithelioid cells themselves. In making this statement only young tubercles and those in which the bacilli are present in moderate numbers are referred to. When there are enormous masses of bacilli, or where there has been confluence of tubercles forming a largish tuberculous deposit, some bacilli may be found in the outer part of the tubercle, but the great majority of them occupy the epithelioid tissue. When the bacilli are few in number, one need only look for them in the epithelioid cells. Around the epithelioid cells the tissue becomes inflamed, and converted more or less completely into granulation tissue. As the tubercle becomes older the epithelioid cells at the centre undergo caseous degeneration, and in this case the bacilli are present in the caseous mass; but are often best seen at its margin, where epithelioid cells still exist, and they may also be found penetrating into the inflammatory tissue. The giant cells of tubercle can be distinctly traced as originating from epithelioid cells, especially from epithelioid cells containing bacilli. As to the origin of these epithelioid cells in the lung, the great majority are derived from the alveolar epithelium. The bacilli escape from the blood-vessels or lymphatics and pass into the alveolar epithelium, where they grow and cause multiplication of the epithelial cells until the alveolus becomes completely filled with them. In some instances, however, those cells are probably derived from the endothelium of blood and lymphatic vessels. In the case of the liver, the author thinks they are frequently developed from liver cells, for bacilli have been found in liver cells at the margin of commencing tubercles, and gradations in size and form can be traced between those liver cells and the epithelioid cells in the centre of the tubercle. The accumulation of the epithelium in the centre of the nodules leads to obliteration of the vessels around and to fusion of neighbouring nodules.

With regard to phthisis, the two extremes, the rapid phthisis or caseous pneumonia, and the chronic or fibroid phthisis, are considered. In the rapid phthisis the alveoli are distended with caseous material, or in parts where the process is less advanced with epithelioid cells. Surrounding these, the trabexlæ are thickened, and converted into granulation tissue. Here the bacilli are found in moderate or considerable numbers in the caseous material and epithelioid cells which fill the alveoli. By-and-bye the walls of adjacent alveoli disappear, and thus irregular cavities are formed containing caseous material surrounded by epithelioid cells and inflammatory tissue. In this case the bacilli are most numerous, and sometimes in enormous masses at the free margin of the cheesy material, and they are also present, though not as a rule so numerous, in the epithelioid cells at the line of junction of the caseous mass with the surrounding tissue. In fibroid phthisis the bacilli are, as a rule, extremely few; but here and there if a cavity exists, or in the centre of a caseous mass, one may find them in considerable numbers. They may, though very rarely, be also found in the giant cells, which are generaliy pretty numerous among the fibrous tissue. As a rule, however, the bacilli are extremely few, but nevertheless, if a sufficient number of sections be carefully examined, a few will be found here and there at the margin of or in the caseous masses.

The foregoing facts seem to indicate that when the tubercle bacilli reach the alveolus of a lung which is in a suitable condition for their growth, they develop in the epithelial cells lining the alveolus. This alveolus becomes filled with cells, neighbouring alveoli become affected, and the same process goes on in them. The further result will depend on the number and rapidity of growth of the bacilli, and on whether the patient is a good soil for their development. If they develop well we have caseous pneumonia; if they grow slowly and with difficulty we have fibroid phthisis. In the former case the alveoli become early distended with epithelioid cells, inflammation of the walls of the alveoli ensues, the epithelioid cells soon undergo caseous degeneration, and the presence of the masses leads to atrophy or sloughing of the walls of the alveoli. Infection of neighbouring parts of the lungs occurs by continuity, and also by partial coughing up and re-inhalation of the bacilli into other parts of the lung. In this rapid phthisis fibrous formation around the alveoli only takes place imperfectly, and the lung rapidly breaks down. In the case of fibroid phthisis the bacilli are few, and grow only with difficulty. Thus fibrous formation occurs extensively, and giant cells are entangled in the fibrous tissue. In parts, however, the process may be more rapid, and there cheesy masses are formed, which

may lead to breaking down of the lungs and the formation of cavities.

In the report it is pointed out that on this view we have one explanation of the rarity of acute tuberculosis in connexion with phthisis, and of the presence of bacilli in sputum, even before physical signs are marked, while it is shown that this view is directly corroborated by the results obtained by Tappeiner in his inhalation experiments. Against the statement that phthisis is due to the tubercle bacilli might be urged the fact that the bacilli found in the lung after death are often very few in number. Among other facts brought forward with regard to this question, it is stated that extensive tuberculous processes may be found in animals, containing only few bacilli, and that in cases where bacilli alone were inoculated, and where it is certain that the bacillus was the only agent at work. With regard to the production of phthisis by the inhalation of dust of various kinds, it is pointed out that the foreign particles inhaled probably only prepare the lung for the reception of the bacilli; for in these cases also bacilli are found. It has often been urged that the milk of tuberculous cows is infective. This may be the case when the mammary glands become tuberculous, and the mode in which the bacilli might get into the milk was well illustrated by the appearances found in a tuberculous kidney. There not only were bacilli present in the tuberculous mass, but they were also found in large numbers in the epithelium of the kidney tubules and in the interior of these tubules, both in the vicinity of the mass and at some distance from it. The author has not yet investigated the subject of tuberculosis of the kidney, but from what he has seen he thinks it probable that the epithelium of the tubules is the favourite seat of the bacilli in the kidney, just as the alveolar epithelium is in the lung. In that case bacilli would be present in the urine, not merely when there were marked tuberculous masses in the kidney, but also where the disease was but slightly advanced. From analogy it is probable that the same is the case in the mammary glands, and bacilli might be present in the milk, even though the disease of the gland was not sufficiently advanced to be noticeable.

The staining solution employed was the Weigert-Ehrlich solution. The formula is: of a filtered saturated watery solution of anilin one hundred parts, of a saturated alcoholic solution of the basic anilin dye (methyl violet, gentian violet, fuchsin, &c.) eleven parts. Mix and filter. Rapid staining is obtained by warming the solution. The specimens are then decolourised by immersion in nitric acid (one part to two of water), and stained in a suitable contrast colour. Very delicate sections are apt to be injured by immersion in the nitric acid. In this

case, after staining them in the Weigert-Ehrlich fuchsin solution, they may be washed in distilled water, immersed in alcohol for a moment, and then placed in the following contrast stain for one to two hours: distilled water 100 ccm., saturated alcoholic solution of methylene blue 20 ccm., and formic acid 10 min. Whenever it is possible, however, Ehrlich's original method is recommended, as being most rapid, most simple, and most satisfactory. By this method of staining, tubercle bacilli and leprosy bacilli remain red. Psorospermiæ and the outer coat of some parasites also retain the red colour. Lichtheim has further stated that a micrococcus is frequently found in the fæces, which reacts in a similar manner to the tubercle bacillus. —*Lancet, March* 17, 1883, *p.* 444.

24.—ON TUBERCULOSIS OF THE FAUCES AND PHARYNX. By Dr. T. Gurovitch (*St. Petersburg Dissertation*, 1882).

The author's inaugural thesis is an interesting contribution to the study of the subject, and is based on a careful examination of eleven cases of his own, and of those found in literature. In common with Isambert and Mackenzie, Dr. T. Gurovitch admits, as a fact well established both clinically and microscopically, that tuberculosis may occur in the fauces and pharynx primarily, though in a vast majority of cases it is developed here only as a complication of advanced pulmonary phthisis. Examination of a patient suffering from primary faucial tuberculosis (and generally showing all signs of pronounced phthisical habit and broken health) reveals an extreme paleness of the mucous membrane, with yellowish tint, and intumescence of the soft palate, lateral walls of the pharynx, and glosso- and arytæno-epiglottidean folds, which present a grey tallow-like infiltration, and are as if strewn with sand. Over the whole faucial and pharyngeal mucous membrane there are scattered minute grey and yellow nodules, which at some spots form small heaps and patches, showing a tendency to ulceration. On the tonsils and pillars there are visible small shallow elongated ulcers, with greyish surface, surrounded by a reddish areola. The isolated erosions are almost invariably of equal size, but they tend to coalesce and to form extensive irregularly defined ulcerated areas. In some cases the whole posterior wall of the pharynx is occupied by a superficial ulcer of this kind. When the process deepens, these erosions are transformed into typical tuberculous ulcers with uneven bases, covered by atonic granulations and grey or yellow deposit, and with irregular ragged edges. In some rare cases the affection begins like common sore-throat, with considerable tumefaction and hyperæmia of the velum and pharynx, tubercles appearing

only later. When faucial tuberculosis occurs in the course of pulmonary phthisis, there usually appear only very few typical ulcers (sometimes only one); but if the fatal issue approach slowly, they may lead to an extensive and deep destruction of tissue. Thus, in one of the author's cases, an ulcer spread on and perforated the hard palate. Not very seldom (in three of eleven cases) tubercles are developed in the fauces and pharynx very late (during the last days of a phthisical patient's life) and then are detected only *post mortem*. The author gives a detailed account of his histological investigation of numerous specimens taken from ten cases, and comes to the following conclusions. 1. In faucial and pharyngeal tuberculosis there occur tubercles of three varieties: the round-celled, epithelioid, and giant-celled. 2. The epithelial covering of mucous glands undergoes fatty and colloid degeneration, but takes no part whatever in the development of giant-cells and tubercles. 3. The subjacent muscles undergo interstitial inflammatory changes with fatty degeneration of muscle-substance. 4. Both glandular and muscular changes, as well as those in connective tissue, are of a diffuse character. 5. Beside Koch's bacilli, the presence of which in tuberculosis does not admit any doubt, there are found, amidst elements of tubercle and especially within giant-cells, some round micro-organisms, which also may play a *rôle* in the development of tuberculous process. (A case of miliary tubercle of the pharynx is reported in the Record, Feb., 1880.)—Dr. V. Idelson, *London Medical Record*, *Feb.* 15, *p.* 34.

25.—MODERN THEORIES AND TREATMENT OF PHTHISIS.

By James Edward Pollock, M.D., Consulting Physician to the Hospital for Consumption, Brompton.

The well-known teaching of the modern school, which I will briefly describe, is that the primary block of the lung is due to pneumonia or inflammatory products.

Catarrhal pneumonia begins in catarrh of the smallest bronchi, extending to the alveoli, which become packed with exudation rich in young round cells. The acute cases are a frequent result of measles and hooping-cough. Under favourable circumstances the cells fill with fat-globules and disintegrate, and the contents of the alveoli become fluid and are absorbed or expectorated. In less favourable cases, the lung tissue becomes consolidated, the cellular element increases in the alveoli, fatty changes are incomplete, the cells lose their rounded form, and shrink into irregular shapes; this represents cheesy transformation. Every form of pneumonia may end in this necrosis of the cells and caseation. In this process the walls of the alveoli are pressed on, their blood-vessels become

compressed, and the walls are broken down, and the form of destruction of lung tissue with which we are familiar takes place. In chronic catarrhal pneumonia a formation of connective tissue fills up the collapsed cells, so that the lung becomes tough and impermeable to air. The pleura is thickened and adherent, and from it bands extend throughout the lung, contracting its volume as the tissue shrinks; the whole side is drawn together, and the bronchial tubes become dilated, giving rise to the phenomena of cavity. These are the more chronic and favourable cases. The diaphragm is drawn up and the heart displaced in their advanced stage.

But the two points on which the German and French schools are at issue are the influence of cheesy transformation of the morbid products in the lung and the secondary eruption of tubercles in chronic cases of phthisis. It seems acknowledged on all sides that cheesy deposits, in breaking up, give rise to or are followed by an eruption of miliary tubercle, either in the neighbouring part of the lung or in the opposite lung. Laennec described this, and it was known that it is at the period of softening of such masses that the appearance of fresh tubercles occurs. Thus cheesy transformations in some part or other originate tuberculosis, which is in this instance a secondary disease. Cheesy masses in some way infect the system. Buhl says that miliary tubercles constantly depend on pre-existing cheesy products. Tuberculosis is an infectious disease caused by reception into the blood of the tubercular poison. He compares it to pyæmic septicæmia. Laennec knew that a secondary eruption of tubercle takes place in a lung already broken down by primary disease: he called it "secondary"; Niemeyer called it a "complication"; but both referred it to a previous caseation of the mass in the lung, which the German said was inflammation, but the Frenchman "tubercle." In either case we see it was regarded as an infecting agent, and this is important as leading on to more modern views still. I must beg your patient indulgence in this recapitulation. We are taking note of the successive theories of phthisis; we are watching the evolutions of thought in observers at different dates; and we must remember that all this time phenomena other than those of tubercle were being equally studied, and old doctrines of disease overturned.

In examining the relations of scrofula to phthisis, we shall find that cheesy transformation of the products of slow inflammations of lymphatic glands is held to be an infecting agent, and that such matters carried in the blood, or by the lymphatics, are deposited in distant parts, and there, as in the lung, originate phthisis, or in another lymphatic far from that originally affected. Thus, we have a step in the direction of blood-

poisoning—septicæmia—or at least that morbid products are so carried and deposited, for septic they are not. If this be the origin of phthisis, the inflammation theory is interfered with; but, again, what is it which has originated the scrofula which started the lymphatic enlargement? Are we then driven back to the old "diathesis," or "constitution," which covered so much ignorance on our parts? In pursuing this line of thought, we are compelled to ask why some persons get catarrhal pneumonia—that is, a block of a portion of lung by inflammatory products which will not liquefy nor be absorbed, but undergo cheesy degeneration,—and others get croupous or sthenic pneumonia, by which a fibrinous exudation is poured out into the alveoli of the lung, nay, of a whole lung, and in the course of fifteen or twenty days is wholly liquefied or cleared out, leaving the delicate structure of the alveoli unimpaired? These two diseases are so unlike in their morbid products, in their symptoms, and in their results and sequences, that they have no right to the same name. Indeed it was in an evil hour that such identity was stamped on them, for they do not possess any point in common, excepting that of their seat. They both affect the lung, but this may be said of several other disorders.

The German school will not acknowledge "constitutions," or delicacy of system, leading to those deposits which will not clear up, but go on to poison the system, and produce like localisation in other organs. They say such persons have a "vulnerability"; that tuberculosis is "not heritable," but that "the disposition to it is." But we know that lung attacks leading to cheesy infiltrations occur chiefly in delicate and badly nourished persons, whose inflammations tend to an abundant production of cells, and thereby to cheesy metamorphosis, and that cheesy metamorphosis anywhere may lead to tubercle. So much for the nature of the product which blocks the lung, and which is known to end in phthisis. We ought to distinguish four kinds: (1) The inflammatory exudation, which is not tubercle; (2) the miliary tubercle, which is secondary and rare, as the Germans say, but primary and common according to Laennec; (3) the cheesy transformation, which is common to all; and (4) the developed connective tissue, which contracts and hardens the lung into a fibroid state.

There is another consideration to which I must draw your attention, and that is the localisation of the morbid product in the lung; and it does not appear that this has received sufficient attention. I do not allude to the lobar or lobular arrangement of deposits, nor to the interesting question why the apex of the lung suffers most in phthisis, but rather to that portion of the lung structure which is the seat of the

morbid product, be it inflammatory or purely tubercular. I take it that here will be found points of difference in the purely inflammatory as compared with the other forms of structural injury to the lung. Perhaps we may make this more clear by asking why it is that lung mischief is so productive of ultimate injury to the individual, giving rise to fever and wasting and all the pernicious results of phthisis? If it were due merely to the amount of pulmonary space lost, the ordinary croupous pneumonia which blocks with the greatest rapidity a large portion of the lung would give rise to these symptoms. But the phenomena of acute pneumonia are altogether different from those of phthisis. We have a sudden attack, very high temperature, and an exudation of a fibrinous character into the alveoli of the lung, in many cases blocking the entire organ — even the opposite lung may be attacked in sequence, and the respiratory space be lost over almost the whole of both sides,—and yet the patient recovers. In practice we say that if the heart be strong enough to drive the blood through the obstructed lung, and does not fail in the effort, the patient may get well. Especially is this the case in that turn or crisis of the disease when the exudation begins to break up and liquefy. The case might almost in practice be called a heart, and not a lung, problem; and those who support the circulating force at the heart will have the greatest number of cures. The lung is not diseased in its structure during such an attack, its terminal cells are filled up, but that is all, and we know that perfect recovery may and does daily take place, the delicate elastic structure of the alveoli being found entirely unimpaired after the exudation has been removed.

Now compare this with the smallest exudation or product of inflammation which has broken down the alveolar walls and infiltrated the peribronchial and interlobular tissues, and we shall see at a glance wherein the difference lies. So long as the alveolar walls are not broken down there commonly is recovery, but infiltrations into the proper lung tissue are not so recovered from. The results in the latter case are not liquefaction and absorption of the exudation, but first mechanical pressure on and strangulation of the nutrient vessels of the lobule, causing the death of the part, so that the infiltrated material is compressed, the alveolus collapses, and those changes are initiated which end in cheesy transformation. How is it that a pneumonia which is not resolved after about two months becomes a phthisis? We believe that it is because in the very earliest phase, that of exudation, the interalveolar tissue has been invaded by the inflammatory product, and that we are not dealing with a portion of lung which has its alveoli filled up with fibrin, but a portion where the alveolar walls have given way and a

diffused deposit has solidified the whole lobule and its surroundings. This is not a croupous pneumonia at all, it is an insidious deposit in the lung tissue, and will have a different ending from that of sthenic pneumonia.

As I have commented on the errors of the French school as seen from one point of view, so it must be remarked that the German has fallen into errors easily pointed out. Having the great task of enunciating new and true doctrines regarding the inflammatory nature of many products in the lung hitherto called tubercle, it was not unnatural that they should have been led into the common exaggeration of making all phenomena of phthisis harmonise with the new theory. Thus the common origin in catarrh was implied when the name was adopted; the term was no doubt given just as pneumonia was forced into the service in order to inform the medical mind once for all that the exudation in the lung was of inflammatory origin, and must have originated in the ordinary causes of irritation of the air passages of which cold and exposure are the chief agents. But we have seen how unlike a true pneumonia is to the so-called catarrhal variety in its seat, the nature of its products, and in its termination; and so we may say of the term catarrhal, which has misled many a practitioner, and induced a prognosis not verified by the result. Its influence on treatment must also be remembered, and the question will arise whether what is suited to a catarrh is applicable to a phthisis. In adopting this word we appear to have gone back to the old vulgar idea that all consumption came from a neglected cold, and that hot and close rooms and means to relieve bronchial irritation are the best remedies to adopt.

There is another statement of this school to which I must advert, and it will be remembered that my object is not to subvert any doctrines, but, as will be seen later on, to show by a study of the changes of opinion on phthisis how a gradual evolution of the most modern teaching has taken place, and to follow the line of thought in the minds of successive masters. Is hæmoptysis ever a cause of phthisis? Is it ever actually the first in the train of symptoms, the one which initiated the disease? Laennec and Louis lay it down that hæmoptysis indicates that tubercular mischief is already established in the lung. The irritation of the tubercle causes congestion, and hence the hemorrhage. Niemeyer says that capillary hemorrhage, bronchial or pulmonary, often induces phthisis even where there was no tubercle, and that in the majority of cases irritation of the lung follows hæmoptysis. The blood which remains and coagulates in the bronchi and alveoli becomes a cause of phthisis. In some rare cases he says hæmoptysis is not a cause, but a consequence, of pneumonic processes which lead to con-

sumption; but the rule is otherwise, and hæmoptysis is most ordinarily a cause of the disease which is to break up the lung. Thus, as regards the earliest stages of phthisis, we have two sets of conflicting opinions. As regards clinical observation, we see a large number of cases initiated by an hæmoptysis. Every practitioner will endorse this observation. The theory of course is that the blood retained in the alveoli, together with pneumonic infiltration, undergoes cheesy change, and this is followed by breaking down of the lung, or by an eruption of miliary tubercle. We cannot forbear from asking the question why the hæmoptysis should occur at all if there be no preceding disorder of the lung. Is it within our clinical experience as a fact? Why should a portion of lung suddenly permit of hemorrhage? We know that the pulmonary structures are capable of undergoing immense strain from sudden and prolonged exertion, and, further, that when a hemorrhage occurs from such causes, or from cardiac valvular disorder, that lung disease, and especially phthisis, does not follow. It is true that hæmoptysis is in a vast number of cases the earliest symptom of phthisis to which the attention of the medical attendant is called, but do we not almost always find that for weeks or months previously the patient has been slightly losing flesh, has felt unusual weakness, has had some suspicious febrile symptoms, or has had slight dry cough? Taking the German theory of a pneumonia as the ordinary cause of phthisis, we can see how the influence of hæmoptysis as a primary cause came to be insisted on. If there was anything morbid in the lung before the hæmoptysis, it was most likely to be what Louis and Laennec said it was—tubercle! And it was necessary to combat this, and to account for the hæmoptysis as the first evidence of the congestion and the inflammatory exudation which were to follow. Here, again, pathology was in advance of clinical observation, for the facts of practice do not bear out the theory. People do not get a consumption because they spit blood, but they spit blood because they are consumptive; and insurance offices and those who advise them are right in attributing that meaning to the symptom. But, in truth, the Germans want to prove too much—their statement that inflammatory products often passed for tubercle is abundantly proved without this attempt to account for a symptom which seems to tell against them. Hæmoptysis is commonly due to something already wrong in the lung, be it tubercle or not, and all experience proves it. In those cases where it is not, it is harmless, and probably due to cardiac causes, or general fragility of the whole vascular system. I have myself records of about three hundred cases of hæmoptysis which did not originate nor result in any disease of the lung.

I venture to recall your attention to the remarkable and able

debate on the connexion of tubercle with phthisis in 1873. At that time we had not fairly reached the doctrines of infection, nor had bacilli been discovered in the sputa of phthisis. The medical mind of this country, which had long reposed on the doctrines of Laennec and Bayle, had been roused by the strong assertions of the German school which I have described, and the result was a declaration of faith on the part of some of our ablest inquirers. The solid observing English mind, which is conservative of old opinions while impartially open to every novelty; which is little given to belief in the dogmas of any school as such, but waits for their verification by evidence, was ably represented by the leading speakers on that occasion. As in surgery it used to be said that operations were invented in France but made safe in England, so the lighter and more ephemeral part of German theories are often reduced here to the level of facts which have been proved by investigation, and if found to bear such crucial test are assimilated with medical science, but, if weighed and found wanting, are rejected. I take it that in no country are the names and doctrines of great names so worshipped as in Germany; but I must maintain that no country is so practical as England.

I will venture briefly to recapitulate a summary of the views of the prominent speakers on that occasion, in so far as they were not contradicted. It was recognised that under whatever name, tubercle or otherwise, it might pass that a product of known anatomical appearances was found in the lung in phthisis; that caseation is not invariably tubercle, but often results from inflammatory products; that a new growth takes place in the walls of the alveoli in all cases of phthisis, leading to destruction of their nutrient vessels; that this does not take place in ordinary acute pneumonia; that caseation was not due solely to inspissation of inflammatory products, but to a new growth in the walls of the alveoli, with which destructive changes in the lung are associated almost uniformly; that if this growth dies quickly, a rapid caseous change takes place, but if slowly, a fibroid change, leading to the well-known alterations in the lung structure that bear that name; that all these changes are manifestations of the disease, and that all occur in the most typical form of acute tuberculosis; that tubercle may occur without inflammation of surrounding tissues, or that it may be secondary to inflammation.

Dr. Wilson Fox states that implication of the alveolar wall is the most constant and typical appearance in the process of tuberculisation. As regards the dispute about inflammation and tubercle, he considers that both may arise simultaneously, and that inflammation may give rise to secondary growth in the alveolar walls, which is a main cause of phthisis, and that the

causes of tubercle may be the causes of inflammation. As regards the nature of the so-called tubercle, he is borne out by Dr. Burdon Sanderson, in considering it to be a lymphatic overgrowth, produced by irritation under special circumstances, anatomical or constitutional, and he believes that adenoid or lymphatic tissue is found in the walls of the air-vesicles, and that as lymphatic irritation may prevail in any part of that system, so it may show itself here. The boundary line between tubercle and inflammation cannot be accurately defined, but there is no doubt that the tubercular growth precedes the caseation. We come now to another element in the case, and that is that certain constitutional conditions are essential to the production of tubercle. Any irritation of the tissue may, in the presence of local or constitutional predisposition, give rise to secondary growths, diffused or circumscribed, which constitute tubercle, and which may be the source of further infection, and that with or without antecedent caseation, although this stage, and that of softening, appear to be most favourable to the change. The real agent in infection is the small-celled growth in the walls of the alveoli.

Again, attention was called to the fact that irritants introduced under the skin of the rodentia give rise to lymphatic deposits in the lungs, liver, and spleen; even the irritation of a seton has done so. It was considered that overcrowded corpusculation in any organ may give rise to similar processes elsewhere, but Dr. Moxon thought that there was no phthisis without caseation, and that tubercle was another phase of inflammation, which latter can cause caseation, and so enter the system. I believe it may be stated with truth that no speaker upheld that common catarrh can originate caseation, and in this view it was pointed out that the initial stage of phthisis is local and not diffused, as catarrh. Finally, it may be said Dr. Wilson Fox spoke the sentiments of the majority of those present when he said, "Tubercle tends to multiply, but can it be produced in the human subject by indifferent caseous products or by any inflammatory change not associated with a peculiar liability of constitution? I strongly doubt both."

It will be seen that the tone of this debate was broad as regards the foreign schools, but accepted the data of neither. You will have noticed also that the whole debate turned both on the nature of the product in the lung and the changes which it undergoes, but also very much on the part of the lung which was so injured. If the doctrine is no caseation or no tubercle, then no phthisis, it is also no less strongly that if there be no new cell-formation crushing and destroying the walls of the alveoli, then also there is no phthisis. The recoverable cases

are those which after inflammatory or tubercular or catarrhal attacks remain with the alveolar walls intact. Destruction of alveoli is never recovered from, there are collapse and thickening of that part of the lung, but there is no restitution of structure and no vesicular breathing in that part again. Localisation of disease rises into great importance. We used to think that the consumptive died of a constitutional irritative fever, but, tracing their symptoms alongside of the physical signs, we now know that their fever and waste are coincident, and that it is just then that the tissues around the alveoli and in the peribronchial spaces are filled with a new cell-growth, which rapidly proceeds to caseation and softening. We also know that at this very time a mass of detritus of inflammatory or tubercular products is being carried into the blood and lymphatics, and deposited in other parts of the system, and that high fever wastes the patient while other organs or the opposite lung is being infected. Whatever initiates the disease this secondary result seems as manifest as the spreading of a fire in a dwelling, while, like the latter, the mystery remains of the originating causes of the combustion.—*Lancet, March* 17, 1883, *p.* 442.

26.—ON THE TREATMENT OF PHTHISIS.—SALICIN.

By Surgeon-General Wm. Johnston, M.D., Northwich.

It occurred to me, as the outcome of the thought I have given to Koch's discovery, that seeing it has now been proved that the bacillus of tubercle is not a mere epiphenomenon of tubercle, but its *vera causa*, one cannot fail to see in it the true source of those infective materials whose existence has hitherto been assumed by pathologists, and which, disseminated by means of the blood and lymph streams, call into being those structural lesions which characterise tubercle as occurring in man and the lower animals. In the presence therefore of such conclusive evidence, is it unreasonable, I would ask, to anticipate that, should the resources of nature and art, as now known to us, fail to supply us with an agent possessing properties adapted to the object of reaching and destroying the microphytic sources of tuberculosis in the deep-seated tissues of the body, such an agent may not be brought to light? That in carbolic acid we possess such an agent in the treatmeut of leprosy, it has been my endeavour to show, whilst at the same time suggesting its possible applicability to that of tubercle. But are our existing resources really so meagre as to leave us destitute of other therapeutic means fitted to subserve the teaching of pathology? Without presuming to answer such a question in the affirmative, it is yet incumbent on me to sub-

mit to the attention of the profession a means which I would suggest is worthy of an extended trial—I mean salicin. Our knowledge of the action of this medicinal agent on the animal body, and of its reactions in disease, remains much as it was when communicated to the profession through the researches of Senator, Lehmann, and Miller. No doubt, in more recent years, additional light has been let in on the subject by other workers whose conclusions on the *rationale* of the action of salicin are certainly suggestive of its being made applicable as a curative means to that family of diseases whose intimate nature and dependence on pathogenic germs the experimental method has placed beyond the reach of cavil or doubt. It is not in salicin, in its simple state, that such a result can be looked for, but in those changes which it most assuredly undergoes while circulating in the blood and lymph streams, where, subjected to the influences of the animal chemistry, it is transformed into agents whose destructive energy on microphytic life none can question. That the resultant of those changes is an acid body endowed with such qualities, abundant proof is not far to seek, though this is not the place to adduce it. Till very recently it was generally believed that this acid was salicylic—a view which is no longer tenable if it be proved, as very recently affirmed by Dr. Stewart, of Edinburgh, and resting on experiments made by himself, that the acid is, in reality, the carbolic. Let this be as it may, be it carbolic or salicylic acid, both agents really belong to a like category, and are both equally available in the treatment of disease of microphytic origin—the former by the method suggested in my letter to *The Times*, the latter by the internal use of salicin, both agents leading to a like result—the destruction of pathogenic germs.

But it is in salicin alone, and not in its compounds, that we must look for the realisation of this object—in its compounds no doubt, but only such as are fashioned in nature's laboratory; and not in the chemist's. Because—and if space were permitted to me here it would be easy to show that the salicylates—comporting themselves as other neutral organic salts, undergo changes in the gastric fluids which involve the destruction of the specific properties of the acid base, while, at the same time, often reacting injuriously on the system by calling into existence symptoms which at times have led to fatal consequences. And, further, it is to be remembered that the success of the treatment is contingent alone on the persistent and the uninterrupted use of the salicin; conditions which will be the more easily fulfilled since experiments have shown, notably those of Dr. Sydney Ringer, that, other things being equal, this substance can be taken in indefinite quantities

and without inconvenience, which would be impossible in the case of the salicylates, even were they much less irritating than experience has shown them to be. The use of salicin must therefore be persistent and continuous in order to balance, so to speak, the equally constant elimination by the kidneys of the compound acids which it forms in the blood and lymph streams; and this, not for the purpose of effecting tissue changes, but for that of destroying the organisms which are the source of those changes.

Let me conclude with the following brief record of a case of tubercular phthisis which has been subjected to the salicin treatment, that of a young man twenty-two years of age, in whom the disease had passed into its second stage when I first saw him a little over two months ago. Then the upper third of the left lung was involved in tubercular disorganisation, while in the apex of the right and subclavian region disease was present and seen in the dulness, increased vocal resonance, prolonged expiratory murmurs, and the compound râles never absent in like cases. For upwards of six weeks the salicin has been persistently used in association with phosphate of lime, with the result that now all disease has quite disappeared from the right lung, while the improvement in the left has been most gratifying. The cough, dyspnœa, and expectoration of mucopus have lost much of their former force, and he can now take exercise without being troubled with his former distressing dyspnœa. He sleeps well, there are no night-sweats, and his appetite is good. A week after commencing the salicin he increased 5 lb. in weight, and he is now stouter, stronger, and looking better than before he became the subject of disease. I need scarcely add that the usual auxiliary means were employed—cod-liver oil, medicated inhalations, &c.—*Lancet*, *Dec.* 9, 1882, *p.* 1003.

27.—ON THE PATHOLOGY OF BRONCHIAL ASTHMA.

By the Editor of the Medical Times and Gazette.

The pathology of bronchial asthma is such a favourite subject of investigation and discussion, especially with our German *confrères*, and the results arrived at by different authorities from time to time are frequently so discordant, that instead of reproducing at once their arguments in these pages, we are compelled to adopt an attitude of interested expectation, and to wait for the confirmation or disproval of the last new view on this as on many other subjects. The most recent contribution, however, to the pathology of asthma appears to us to be so important, and so likely to prove in the main correct, as to deserve to be submitted to the profession in this country, and to

be carefully tested by clinical experience. This contribution comes from the pen of Prof. Reigel, of Giessen (the author of the article on Asthma in Ziemssen's Cyclopædia), and appears in the Zeitschfur Klin. Med. Bd. v, p. 413.

In approaching the consideration of the pathology of asthma, Prof. Riegel was met by several distinct questions which are still more or less imperfectly settled. The first of these questions manifestly is, Is there really such a function in the bronchial muscles as active contraction, sufficient to affect the calibre of the tubes and to modify the pressure of the air within the lungs? Very different have been the answers given to this question by different physiologists—for we must be careful to notice that this is but a matter of physiology, and not of clinical medicine. Prof. Riegel's results are entirely in accordance with the accumulating evidence of the work of recent, as well as of some of the older and most distinguished, observers—namely, that irritation of the bronchial muscles does raise the pressure within the lungs, and that this irritation may be induced through the medium of the vagus. This point having been settled, the next question was whether stimulation of the vagus caused acute dilatation of the lungs, such as is seen in asthmatical seizures; and this question, also, Prof. Riegel was able to settle in the affirmative, the pulmonary area enlarging rapidly when the vagus was galvanised in the neck (in dogs), remaining large during the continuance of the stimulus, and slowly returning to its normal dimensions when the irritation was removed. Nothing could have been more easy, or indeed more natural, than to conclude after these two series of experiments that the pathology of bronchial asthma was practically settled; that this disorder is essentially a neurosis of the vagus, the dyspnœa due to bronchial spasm and the pulmonary dilatation being the direct results of irritation of the great nerve of respiration. Prof. Riegel was too cautious, however, to rush to this conclusion, and his next set of observations showed the wisdom of his hesitation; for they distinctly proved that whilst irritation of the vagus unquestionably produces the phenomena of asthma, *it does not do so by causing spasm of the bronchi.* No doubt, as has just been shown, spasm of the bronchi is a result of irritation of the vagus; but there is a much more important, because much greater or more extensive, cause at work than this.

The turning point in the investigation was the discovery that irritation of the vagus causes the phenomena of asthma, not by acting peripherally—that is, through the branches to the bronchi—but by influencing the central extremity of the nerve, that is, the medulla, and so (reflexly) the muscles of respiration.

When the *central* end of the divided vagus of the left side was faradised, and the other vagus cut, the same asthmatic phenomena were produced; the reflex, therefore, did not occur through the bronchial nerves, but by the respiratory nerves to the diaphragm and intercostals. That this was the case was completely proved by section of the phrenics before irritation of the vagus, for the phenomena of asthma were then entirely absent. An altogether unexpected result was thus reached, namely, that asthmatical phenomena may be produced *reflexly* through the vagus, and that the principal portion of the effect is a sudden inspiratory depression of the diaphragm, followed by its continued tonic contraction. It would thus appear that the theory of asthma, which represents the disorder as essentially one of bronchial spasm, must be given up. There can be no doubt that irritation of the vagus does cause bronchial spasm and moderate dilatation of the lungs, but this effect has always been regarded as much too insignificant to account for the symptoms of the disorder as clinically observed; and now that it appears to have been satisfactorily proved that besides this peripheral effect there is a reflex effect of incomparably greater importance, there is no reason why the theory of bronchial spasm should be any longer maintained.

Two very obvious objections to the view first stated are anticipated by Prof. Riegel. Can it be possible, in the first place, that the diaphragm may remain so long in a state of contraction as to cause the protracted dyspnœa familiar in many cases of asthma? There is no evidence to the contrary; and in the course of these experiments on dogs the diaphragm was actually seen to remain in a condition of contraction for ten minutes without producing asphyxia. Besides, the same objection might apply to the muscles of the bronchi. Again, it is a clinical fact that whilst the inferior lung-border is low in an attack of asthma, it moves in respiration. Is this fact compatible with spasm of the diaphragm? As a matter of fact it is: whether the phrenic be directly or indirectly stimulated, and the diaphragm thrown into inspiratory spasm, the lung-border moves slightly in respiration.

Lastly, Prof. Riegel cautions us against coming to the hasty conclusion that we have now settled the pathology of bronchial asthma. Spasm of the diaphragm may explain some of the phenomena of the seizure, but it certainly will not explain all. For himself, he still holds that there may be vaso-motor disturbance and hyperæmia of the bronchi, along with spasm. Still, spasm there is, and the present investigation shows that it is chiefly a spasm of the diaphragm.—*Medical Times and Gazette, March* 31, 1883, *p.* 335.

DISEASES OF THE ORGANS OF DIGESTION.

28.—ON EUCALYPTUS ROSTRATA AS A REMEDY FOR DIARRHŒA.

By T. J. Hudson, M.B., L.R.C.P.Lond., Resident Medical Officer, Leeds Public Dispensary.

I am induced to bring the above drug more prominently before the notice of the profession owing to the very marked success its administration has met with in my hands in over two hundred cases of various forms of diarrhœa during the past summer. Not that it is by any means a new remedy, though noticed cursorily in the text-books. Eucalyptus rostrata (Australian red gum) was first brought into Europe by Sir Ranald Martin, and occurs as imported in dark-red hardish masses, its essential principle being tannic acid. It is supplied in a pure form by Messrs. Harvey and Reynolds, of Leeds. The preparations found most useful have been a concentrated decoction (strength 1 in 20), and a dilute (strength 1 in 40) made by boiling the powdered gum in distilled water for ten minutes, and filtering while hot, and a syrup (strength 1 in 3). For a moderately severe attack in the adult I commence with half an ounce of the dilute decoction every two hours. If after four doses no improvement results, the same quantity should be given every hour for four times, and if still little effect is apparent, I order half an ounce of the strong decoction every two or three hours. In the vast majority of suitable cases an abatement of the attack now occurs, going on rapidly to a cure, when the same dose should be ordered every five or six hours, only gradually discontinuing the remedy. In an acute case it is best to commence with the strong decoction at the first, half an ounce every two hours, and at times every hour, increasing the time as above. The syrup may be given to children in doses of five to twenty or more drops three or four times a day, but, mindful of the tendency of sugar or mucilage to run into fermentation, seldom prescribe it, preferring small doses of the dilute concoction (thirty to sixty drops) guarded with spirits of camphor or some simple carminative every few hours, pushing it if needful. In many of the worst cases occurring in children, when all other remedies have failed, this alone has effectually stopped the alvine flux. The forms of diarrhœa alone benefited by this drug are as follows—viz.:

1. That arising from want of proper assimilation, the unaltered food causing irritation, chiefly of use after this or other harmful substance has been removed from the alimentary canal. 2. The bilious. 3. The congestive, an inflammatory state of mucous membrane existing, most useful in the latest

stages, that the result of sewer gas also coming under this head. 4. Summer or sporadic cholera. 5. That the result of amyloid degeneration of the intestines, usually the small. 6. The chronic or white flux; of great service in this variety.

The above applies equally to children. In cases of intermittent diarrhœa, the patient having an attack every two or three days for some time, while well in the interval, the drug is very effectual, given as above twice or thrice daily. It is also of service in those cases which, when first seen, present great depression, where the offending cause must be allowed to take its course, and where opium is contra-indicated. The good results obtained are not simply owing to the tannic acid contained therein, as many cases unaffected by the latter are soon cured by the red gum, partly, no doubt, owing to the far less irritant properties of the decoction. It is contra-indicated where there exists much acidity or flatulence, and if the griping pains are very severe a few drops of laudanum may at first be added with advantage, but omitted so soon as this symptom is in abeyance. It adheres firmly to mucous surfaces, diminishing their secretion, coagulating the albumen, the uncombined portion serving to constringe and contract the vessels of the gut, and to give the latter tone.

As tannic acid diminishes the solvent power of gastric juice, the gum should not be given too near food. Its taste is rarely objected to, and can be improved by the addition of spirit of chloroform. Lastly, its cheapness is a desideratum, more especially in dispensary and hospital practice.—*Lancet*, *Dec.* 16, 1882, *p.* 1029.

DISEASES OF THE URINARY ORGANS.

29.—ON RENAL INADEQUACY.

By ANDREW CLARK, M.D., LL.D., Physician and Lecturer on Clinical Medicine, London Hospital.

I think that disease expresses itself in three different ways. First, we have disease expressing itself by what we call mere alteration of function, in which state, by no means at our command at the present time, can we discover the smallest change in the structure; and that is what I call functional disease. In the second place, disease expresses itself in visible changes of structure, which are temporary only, and not permanent. For instance, take hay-fever. I believe myself that hay-fever is preceded by molecular and chemical disturbances; and that, at a certain stage of these, there arises suddenly a swelling of the mucous membrane of the nose, together with the production of a viscid irritating secretion, which gives rise to sneezing and

the other phenomena with which we are all familiar. And, lastly, there is a third aspect of disease, in which, with the preliminary physical or chemical changes, or both, with the transitory structural changes, there comes at last a permanent change: as, for example, cancer or tubercle. Take, for a moment, the case of cancer of the stomach. Years before cancer of the stomach appears, you will find in some of those people, that they are subject to pain or other distress. Somewhat later in the day, they are subject to recurring attacks of catarrh of the stomach, as it is called—states of the stomach in which the mucous membrane swells up, producing viscid irritating secretions, giving rise to what we call forms of indigestion; and then, after treatment, or independent of treatment, it subsides. This will go on for years, and by-and-by growth riots over development, and at last a permanent structural change appears, and we have what we call the third aspect of the way in which disease manifests itself. There is the first stage, and it is the first stage to-night to which I wish to allude, in which we have symptoms of disease without any visible alteration of structure discernible by any means at our command.

Multitudes of people are constantly suffering from ailing health, whose ailing health cannot be referred satisfactorily to any definite cause. The progress of knowledge is greatly enlightening our ignorance about these multitudes of people. It is telling us that some of these people owe their troubles to heredity, to the father who has "eaten sour grapes, and set his children's teeth on edge," to a capricious, weak, and irritable nervous system, to violations, petty but continuous, of simple physiological laws, and so on. But there are numbers of this multitude of ailing people whose illnesses cannot be accounted for by any one of these causes to which I have alluded. Upon this multitude I would venture to make a little inroad, and I say that one considerable part of that multitude owes its ill health to deficient excretion.

We all know the importance to the solidarity and health of the economy of an adequate production and discharge of sweat. We know equally the importance to the economy of the adequate production and regular discharge of fæces. I am confident, with respect of this latter part of the question, that numbers of young women owe their anæmias, their chloroses, their defects of health of various sorts, and often of long duration, to the inadequate discharge from the body of fæces; and I now well understand that which one of our great fathers in medicine is reported to have said, speaking of the treatment of anæmia, that, if he had only two remedies to deal with anæmia in young women—purgatives and iron—he would prefer to rely for success on his purgatives. Well, the same thing may be

said about defective excretion from the kidney. The kidney, as we all know, has a very important function to discharge, and the well-being of the economy seems, in a very peculiar and definite way, to depend upon the adequate discharge of the kidney's offices. We know that what is called Bright's disease is not, as a rule, a structural alteration in the kidney which brings about at last a fatal issue, but it is interference with the function of the organ, interference with the eliminative function of the organ; it is not simply the letting out of albumen, for similar amounts of albumen and greater amounts of albumen in other affections can be discharged from the body without serious detriment to its well being; it is interference with its excretory function; it is the retention in the blood of matters which ought to be cast off from the blood, and which, not being cast off from the blood, remain in it and poison it. There are cases which, in some measure, I venture to separate off entirely from Bright's disease, in which the kidney, without any sensible alteration of structure that our modern means of investigation will enable us to determine, cannot produce a healthy urine. Such kidneys produce a urine which, assuming the quantity to be a quantity of health, is low in density, and is deficient in solid constituents, principally the constituent of urea and its congeners. I will exclude uric acid. I have spoken on this subject once before, and, on that occasion, I committed an error. I said that the main feature of a kidney in this state was, that it could not secrete urine with a proper amount of urea and uric acid; but I find that many of these kidneys, of which I shall speak more precisely in a moment, do not have a deficiency of uric acid; I have learned that since. They have all, however, a deficiency of urea. There is a certain state of the kidney, I repeat, in which, without any alteration of structure that the eye can detect, it can, nevertheless, not produce a perfectly healthy urine. It is an urine low in density and deficient in solid constituent, principally in urea and its congeners. I call this state renal inadequacy.

You may say, "It seems scarcely wise to introduce a name like that, when probably it is nothing less than an early stage of Bright's disease. Why bring in another name?" I will not say that it is not an early stage of Bright's disease; I do not know. I think it need not necessarily be; but I shall assume that it is, perhaps, a very early stage of Bright's disease. I nevertheless think it of practical value—and we who are here to-night are practical men—to recognise by a distinct name a state which may remain as it is during the whole period of life, which is nevertheless capable of removal, and which, if unnoticed, may lead to serious injury to the patient.

Let me explain. The people who have this renal inade-

quacy are characterised by three things particularly. First and foremost, they are characterised by a curious inability properly to repair damages done to them either by accident or by disease. I have no doubt you, as well as I, have often been puzzled to know why, in particular cases, they could not repair a common accident; or why, in a disease such as pneumonia, the exuded stuff was not melted and speedily swept away; why a man who had met with some trifling accident in the wrist or shoulder remained suffering from it. Then, they not only repair damages of this kind slowly, but they are peculiarly vulnerable. They are a people, as a rule, who are always catching cold, and who, when they catch cold, come within the category of the first characteristic—namely, that they do not get rid of the cold. They are the people who, without apparent reason, and without other existing disease, get pneumonias, pleurisies, pericarditis, and the like. Then, thirdly—and, I think, almost the most important thing to be noticed about these cases—you can never be sure of the result of the performance of an ordinary surgical operation upon them. It is this class of people, as I had the opportunity a few years ago, in London, of discovering, that die from a simple operation by hemorrhage. It is this class of people who have an abscess opened and immediately become what is called pyæmic. It is this class of people who, without being able to explain it, attracted the notice of that distinguished surgeon, Sir James Paget. Some years ago he said, "Whenever I find a man in ill-health, without definite cause for the ill-health, I feel sure that my chances of success in operating upon him are diminished by at least one-half." Subsequently he found out, and recently expressed the opinion to me which I had expressed upon a patient of his, that it was due to the low density of the water. The story is this. I was summoned to see a man who was about to undergo a surgical operation, and I was asked the question, "Is this man a suitable person for a surgical operation?" I required, before pronouncing judgment, that I should have the complete urine of two days kept and sent to me. I examined it. The urine was under the quantity —under forty ounces; the density was 1008, or thereabouts: there was no albumen; there were no casts; but the urea was 1.2. I replied, that it would be at the peril of his life if any operation were performed upon him. They were not satisfied —at least the surgeon was not—with this expression of opinion. Sir James Paget was called in, and he gave the same judgment. Meeting him afterwards in consultation, he asked me the question, "Why did you object to an operation being performed upon the patient?" I said, "On account of the state of the urine;" and he said, "I objected also on the same

ground." That shows that from different quarters this question of renal inadequacy has presented itself to the surgeon's mind as well as to the physician's. This, then, is what I mean by renal inadequacy. I refer to those people who have kidneys which, though not materially altered, or at least not altered in any way that we can determine by physical investigation, are yet incapable of producing a sufficiently healthy urine (like an imperfect skin, that is incapable of producing sweat); I mean an urine sufficiently rich in the ordinary matters of waste which it is the business of the kidney to discharge from the body.

Now, how are you to know these cases? How are you to discover them? Here I am bound to say, that I know of no distinctive symptoms whereby, in the early stage of renal inadequacy, you can discover these cases with certainty. The patients are ailing people. I began by saying that there were before all of us, in our daily life, multitudes of ailing people, the explanation of whose cases was hidden from us, and I said that I wanted to make an inroad on that multitude, and pull out from it a number of these renal inadequacy cases. Now I have to confess that, in the early stage, I know of no symptom whereby you can with certainty detect these cases. The only thing I can say is this. When you get hold of a patient who is ill, suffering sometimes from dyspepsia, or nervousness, having headaches or complaining of *malaise* and weakness, who cannot sleep well, who cannot do his work very well, examine his urine, and if you find that the urine is low in density, you had better proceed a little further, and be very precise, and get the urine of twenty-four hours; and if you find that it is under fifty ounces in quantity, that it has not a specific gravity of 1010, and that the urea in it is deficient in amount—under 2 per cent. —then, whether there be albumen in the urine or not (I am not now speaking of albuminous urine), whether there be any casts or not, whether there be granular *débris* deposited or not, you may know with certainty that the kidney is not doing its duty. Well, it may not be, although these kidneys are not doing their duty, that the defect is the cause of the patient's ill-health. How are you to ascertain that? You can sometimes, not always (always does not happen to honest men), ascertain it in this way. If you give a patient a liberal diet he gets worse; and what is the strangest thing I know about these cases is that, if you give a patient a very liberal diet, namely, food and wine, the specific gravity of the urine, instead of increasing, as you would expect, diminishes in density. Furthermore, you may discover that the renal trouble is the cause of their symptoms if you notice in them—and you can notice it very often—that a diminution in the bulk of the urine is always at-

tended by an aggravation of their sufferings. And there is one other way, which is the other surprising thing about these cases, that if you diminish the quantity of food, if you make the patients careful about their ingoings and careful about their outgoings, you will see at once a great improvement. I have, for example, a case in my mind's eye, which I saw with two or three doctors some time ago. The patient was a man who was extremely distressed, short of breath, with palpitations, headaches, and great distress of body in various ways; he had a weak heart also. The doctors who had seen him said that he wanted keeping up. Being a rich man, he was kept up, and he was no better for the keeping up. He had meat and wine in plenty, but he got weaker and weaker. He was then put upon a starving plan—that means, upon a physiological plan, upon a plan which would give him just enough good food for the maintenance of the body and no superfluity, with enough water to form an abundant vehicle for the chemical operations which are necessary, not only for the formation of waste matters, but for carrying them off. He was put upon an old woman's diet, three meals a day; tea and toast for breakfast, a midday dinner, with a little animal food, and tea and toast for tea—a John Abernethyan diet. In a very short time the urine, which had gone down to 1004, began to rise; instead of getting lower and lower in density with the diminished diet, it got higher and higher, and the patient got better and better—not quite well, but pretty well. These circumstances with reference to the state of the urine, the diminution of the urea, the fact that the increase of food makes the patient worse, and that, within certain limits, the diminution of foods makes him better, with attention to the secretions—these circumstances put together will soon enable you, I think, with sufficient accuracy to determine what is the sort of case that you are dealing with; and I venture to think that the knowledge which you will acquire as you go on will be sufficiently useful to you to justify me in having presumed to occupy your time for a few moments to-night. You will know one or two things about these patients which, for their safety and their well-being, it is well to know. You will know that these people cannot be operated upon with the ordinary chances of success. You will know that if they take cold you must treat the cold in them as a serious thing. You will know that if they are ill, that if they have a headache or pneumonia, you will be able to warn the patient's friends (and foresight is an important thing in medicine) that they may not recover immediately from the effects of the disease—nay, more, that the unabsorbed pneumonic products may remain to be centres and sources of future mischief. These are important points.—*British Medical Journal*, *Feb.* 24, 1883, *p.* 345.

30.—ON URIC ACID: ITS RELATION TO RENAL CALCULI AND GRAVEL.

By A. B. Garrod, M.D., F.R.S., F.R.C.P., &c., Consulting Physician to King's College Hospital.

Healthy human urine consists of water, holding in solution both organic and inorganic principles; the former are, urea, uric, and hippuric acid; the latter, chlorides of different metals, especially sodium, and phosphates of sodium, magnesium, and calcium.

It is to uric acid that we must chiefly direct attention; and I must endeavour to show (1) in what state of chemical combination it exists in the urine; (2) why it is held in solution in an acid fluid; (3) what are the causes which lead to its precipitation from the urine; and (4) what are the different shapes which it assumes when it is thus precipitated from its state of solution.

1. At the present day I believe it may be asserted that uric acid, when in solution, is combined chiefly with sodium, but that there are also varying quantities of other bases present, depending on the amounts of different salts contained in the urine. I have already shown that when urate of ammonium is dissolved in blood serum in which the soda salts are present, it is converted into urate of sodium; and, bearing this fact in mind, we can explain the discrepancies which are found in the different analyses of urate deposits, both in health and disease. I may, however, say that the deposit which until recently was commonly called lithate of ammonia is composed almost entirely, in healthy urine, of urate of sodium. If, however, the urine at any time becomes ammoniacal from the decomposition of the contained urea, then the uric acid, meeting with a large excess of the new-formed base, gets deposited as urate of ammonium, a salt which is very insoluble.

2. The next point to be considered is the reason of its existence as urate of sodium in a fluid having such a well-marked acid reaction as healthy urine. This fact was for a long time difficult of explanation; but Liebig showed that if to a warm solution of the common phosphate of sodium, which has an alkaline reaction, uric acid be added till it no longer dissolves, the solution becomes strongly acid, and there is contained in it urate of sodium and the acid phosphate of sodium, which latter salt exhibits a full acid reaction, but does not possess the power of precipitating the uric acid. This phenomenon solely depends on the tribasic character of phosphoric acid, which allows of a solution of phosphates, which reddens litmus powerfully without containing any free acid.

3. When, however, the least trace of a free acid, even acetic, exists in the urine, the whole of the uric acid is rapidly precipi-

tated; a fact of considerable importance in the study of diseased conditions of the urinary excretion. If our attention is directed to the subject, we see almost daily, that, when urine is kept for a time, perhaps only a few hours, the uric acid, which at first was in complete solution, becomes gradually deposited in the crystalline form—a change due to the generation of a free acid in the urine by the occurrence of what is called the acid fermentation.

4. Our last point is to ascertain what shape the uric acid assumes when it becomes insoluble, and is precipitated from the urine. It may be thrown down either in combination with a base—that is, in the form of a urate, or as free uric acid. When as a urate, it is often from simple concentration, or from the presence of too small a quantity of water in the urine, and it seldom happens that such deposition takes place in the urinary organs themselves unless there is a something present, such as a foreign body or the nucleus of a calculus, which greatly facilitates it. When, however, such urine is removed from the body and cooled down to the temperature of the air, more especially in cold weather, the appearance of turbidity is extremely common, and often becomes, though most unnecessarily, a source of great mental disquiet to patients. When the urine, either concentrated or not, becomes abnormally acid, it at first causes the urate existing in it to be less soluble, but soon afterwards the acid itself is separated and deposited in the crystalline condition, forming what is commonly known by the name of Cayenne pepper gravel. Let us take what may be looked upon as pure uric acid; dissolve it in boiling water, and then allow it to cool and crystallise; it will be found in oblong tabular crystals, which are both homogeneous and transparent. When uric acid crystallises out from urine, it is more or less coloured, from yellow to brown, and in the form of thin rhomboidal prisms, showing that there must exist in the urine something which causes an alteration not merely in the colour, but also in the crystalline form.

The result of our inquiries, as far as they go, may be summed up in a few words. Perfectly healthy urine should show no appreciable deposit; when, however, it becomes concentrated from deficiency of the watery excretion, then the uric acid is thrown down in the form of a urate. This may occasionally occur within the body, but far more frequently after the urine has been voided; sometimes, however, this change ensues so rapidly that the urine is erroneously supposed to have been passed in that condition. The presence of a solid body in any part of the urinary tract favours deposition very much, and hence urine which would otherwise remain clear may yield a deposit to any substance previously present in the same tract,

and may thus add considerably to an already existing calculus. The appearance of the numerous layers so frequently seen around a central nucleus, both in renal and vesical calculi, is thus easily explained. When, however, the urine becomes further altered in composition—if, for example, a free acid is either excreted with the urine or rapidly generated in it through the setting up of the lactic fermentation, the uric acid becomes liberated from its state of combination, and, in a form more or less altered by the presence of colloid matters, is deposited on a previously existing calculus, or is passed as separate rhomboidal crystals or in aggregated masses, constituting gravel or sand. I should feel disposed to confine the name of "sand" or "gravel" exclusively to such deposits which, I believe, seldom form the nucleus or become the starting-point of any calculus. I may add that urine possessing these characters is frequently voided for months and years without the occurrence of any appreciable inconvenience to the patient. It is true that a calculus may be augmented by contact with such a urine; but, as I have said, it seldom originates in this way.

According to this view, which I feel inclined to adopt, gravel or sand consists of uric acid previously in a state of solution, which has become precipitated by the occurrence of some change in the urinary excretion.

Some individuals pass a larger daily quantity of uric acid than others; but, at the same time, those who pass the largest quantity may have a urine little disposed to deposit the principle; and it will be found, as a result of experience, that changes in the urinary excretion, leading to the crystallisation of its contained uric acid, are much more potent factors in the production of sand or gravel than the mere quantity of this acid which is eliminated. If there is a simultaneous occurrence of the two conditions—that is, of increased quantity of the acid and altered state of the urine,—this facilitates still more the production of the morbid appearances.

Influence of Diet.—There can be little doubt that the occurrence of gravel and calculus is largely influenced by the diet, but on this subject I feel sure that the opinions frequently held are not altogether correct and require to be reconsidered. As we shall find that a gouty diathesis is so potent in the production of the diseases under review, it will be quite safe, in so far as the discussion of food is concerned, to assume that what tends to produce gout tends also to develop calculus, and that the diet which is of avail in the treatment of the one disease is equally so in the management of the other. It will be desirable to turn our attention to the principal groups of aliments and ascertain what influence they have upon the formation of uric acid and also upon its condition with respect to solubility.

Sugar.—The most common of the non-nitrogenised principles contained in food is starch, seeing that it forms 70 per cent. of wheaten flour, and almost the whole of many of the simple amylaceous articles of food, as rice, maize, arrowroot, sago, &c.; also of the potato, turnip, carrot, and so on, when these latter are dried. It can be shown that, when taken into the alimentary canal, starch is soon changed into glucose sugar by the action of the saliva and pancreatic juices; and, when cane sugar is taken, the same change ensues, so that however carefully sugar is avoided as an article of food, it is still abundantly formed in the canal when amylaceous matters are eaten, and the result is the same whether a pound of starch in any of its dietetic forms or a pound of cane sugar be taken.

There is a very popular idea that sugar causes what is termed acidity, and hence it is scrupulously avoided by many. Is this true? Between two or three years ago I was much struck at seeing an American surgeon of great repute putting lump after lump of white sugar into his tea, and I asked him why he did so. He told me that in the States it is a common habit to take sugar thus as a preventive of heartburn, and that he took it for that purpose. His answer made a strong impression on my mind, and since then I have often questioned dyspeptic patients as to their experience on this point. At first nearly all exclaim, "Of course sugar causes acidity," but as yet I have failed to find anyone who could assure me, from personal experience, that the eating of lumps of ordinary white sugar produces more so-called acidity than taking any other article of diet. It must be borne in mind that I do not for a moment include sweetened fruits, and such-like substances, in the same category as simple sugar. One can hardly believe that the eating of a lump of cane sugar would seriously add to the glucose which is daily produced in the alimentary canal of an individual living on an ordinary mixed diet. Let us see what has been found experimentally with regard to the influence of sugar on the production of uric acid. Böcker says that the effect, in man, is to lessen the quantity of that principle, and Bischoff and Voit have proved that, in dogs, starch produces the same effect on the urinary excretion as sugar, so I think we may say that there is no increase in the uric acid when sugar is taken.

I must devote a few minutes to the discussion of a most important subject—viz., the influence of different alcoholic beverages on the production of uric gravel and renal calculi. We must remember that all such beverages contain alcohol united with different proportions of water, some little more than this; others, however, contain sugar together with colouring and so-called extractive matters, also salts of potash and lime united with vegetable or mineral acids. Many wines

also contain a certain amount of some free organic acid. Now, have we any facts with regard to the special effects of different wines in the diseases which we are now considering? I think we have many, and much information which we can use to guide us in the prevention of such diseases. It may, as I believe, be confidently asserted with respect to gout, that, with an absence of alcohol in any shape, coupled with an absence of hereditary predisposition derived from alcohol-drinking ancestors, the disease would be practically unknown.

It is most essential to separate the different kinds of alcoholic beverages from each other in estimating their tendency to produce disease. Thus, alcohol in the form of distilled spirits, although when taken in excess it causes serious mischief, injuring the liver, kidneys, heart and other organs, still has little or no power of producing the uric acid diathesis, or, at any rate, the gouty development of it. In spirit-drinking countries, or among spirit-drinking families, gout is unknown. Look at Scotland and its whisky-drinking classes—the disease is practically absent, hardly ever seen in the hospitals. Look at Poland, where they drink a kind of arrack; the same holds good.

When, however, we investigate the influence of wines we shall find a different result. Drinkers of the common light wines, such as the red Bordeaux and the Rhine wines, suffer but little, while, among the same nations, those who indulge freely in beer are by no means free. The natural light wines, in which the alcohol is small in amount, while there is an almost complete absence of unfermented matter, which contain, also, a considerable amount of acid vegetable salts, are little liable either to produce gout or to lead to the formation of calculus or gravel. On the other hand, the Peninsular wines and those which resemble them, which are stronger in alcohol, contain much unfermented matter, and are almost devoid of the vegetable salts, have great gout-producing power, and lead readily to a condition of urine favourable to the production of gravel and calculus.

We come, lastly, to the malt liquors, ale, beer, stout, and porter. From my own experience, and I believe it is also the experience of all who have attended to the subject, I can confidently assert that these beverages have a great tendency to produce the uric acid diathesis. Compare the hospitals of Edinburgh and Glasgow with those of London; in the former gout is scarcely known, in the latter the disease is common, the difference, as I believe, being chiefly due to the different beverages drunk by the working classes of the two countries; it is, in fact, the difference between whisky and malt liquors.

It is necessary that we should at least endeavour to ascertain what principle or principles, present in some of these

alcoholic beverages, absent from others, lead to the development of this diathesis or aggravate it when it is already manifested owing to hereditary or other causes. It cannot be the alcohol alone; this, I believe, can be fully and satisfactorily proved, seeing that large groups of people whose custom is to drink freely of distilled spirits are yet free; instances are to hand in Scotland, Sweden and Norway, and Poland. It cannot be the sugar alone; for, although the partially fermented wines and malt liquors contain sugar, yet sugar added to distilled spirit does not appear to produce the uric acid diathesis. It cannot be the acidity alone; for the wines which are most harmless are quite as acid or even more so than malt liquors and the Peninsulur wines, and many people who strongly object to the least acidity in wines, will often take lemon juice to an extravagant extent. If, then, neither the alcohol, nor the sugar, nor the acidity by itself is the cause of certain beverages proving so injurious, is it a combination of any of these that does the harm? We already know that the combinations of alcohol with sugar, and that of alcohol with acid salts, are innocuous as far as the uric acid diathesis is concerned. What, then, is there left for us to fall back upon in explanation of the peculiar properties which some of these beverages possess, while others are devoid of them? The only conclusion that I can arrive at with my present knowledge—and it is the result of much thought during many years—is that it is something which is a result of imperfect fermentation, and you will find that it is those beverages in which fermentation has commenced, and has been allowed to proceed to a certain extent and has then been checked, which, of a certainty, cause gout, and probably lead also to the production of gravel and calculus. If I am asked to state more exactly what this principle is, I cannot do so; it may be an influence only, a condition of matter—a ferment.

In connection with this subject, however, I must return for a moment to that of sugar, which, I told you, had, as I thought, been regarded askance without due cause. I would say that I do not for a moment classify with sugar either sweetened fruits or vegetables; for I am quite sure that such articles of diet will frequently produce heartburn and other dyspeptic annoyances in individuals who are not in the least inconvenienced by sugar itself. I cannot help thinking that these contain a something which is not simple sugar, but a substance which is the result of the long contact of the sugar with the fruit or vegetable juices, a kind of semi-fermented matter—in fact, that same "something" which exists in the stronger wines and the various malt liquors. Of this I feel confident, that in many cases where sugar, whether by itself or

in tea, coffee, and light puddings, does not disagree, and where fresh fruit, although sweet, produces no discomfort, the combination of sugar with these juices, if time has been given for them to act upon each other, will often cause well-marked dyspeptic symptoms. But, it may be said, if so, a ripe orange cannot be a good thing to eat, as it contains both sugar and acid juice, and these substances have been in contact with each other for a long time. I answer, not necessarily so; so long as the orange exists as a fruit, with its botanical structure intact, so long there may be no change taking place between its different constituents. We have a striking analogy to this in the case of the bitter almond. When whole this seed contains the crystalline amygdaline and an albuminous ferment. Separate one of these from the other, and each by itself is innocuous; crush and moisten the almond, prussic acid is immediately formed, and the union of the two principles is the production of a deadly poison.

I must now refer to classes of food in reference to the uric-acid-secreting function; and, first, with respect to the influence of a nitrogenised, and especially of an animal diet. Observations are not wanting to show the effects of a pure meat diet on the urine. Those of Lehmann are perhaps the most satisfactory. He first determined the daily excretion of the principal constituents of his urine when on an ordinary mixed diet; he then placed himself on a purely animal diet (chiefly eggs) for twelve days; afterwards, for another twelve days, on a purely vegetable diet; and subsequently, for two days, on a purely non-nitrogenised diet, which consisted of fat, milk, sugar, and starch. His conclusions are that the total solids, as well as the urea, are much increased by animal food, while they are considerably decreased by a vegetable diet, and still more so by one which is non-nitrogenised; whereas the uric acid is not nearly so much affected by the nature of the food, provided that it contains nitrogen. Thus the urea, on an animal diet, was to the uric acid 53·198 : 1·478; on a vegetable diet, 22·481 : 1·021; and on a mixed diet, 32·498 : 1·183. He found that after the use of purely animal food the urine of man closely resembles that of the carnivorous mammals, becoming of a light amber-colour, having a strong acid reaction, and containing neither lactic nor hippuric acid. On the other hand, after a course of vegetable food, the urine becomes of a brownish-red tint, is much less acid, often deposits the earthy phosphates, and always contains alkaline lactates with oxalate of calcium; in fact, the urine closely approaches to that of the herbivorous mammal.

As a result of all the experiments made by different observers, both on man and the lower animals, I think we may fairly

conclude that meat, taken in such quantities only as are sufficient to keep up the nutrition of the body, has no tendency to increase the excretion of uric acid; that, when the diet is purely animal, but the quantity small, the uric acid, far from being large, becomes exceedingly small in amount, more especially when it is compared with the urea. On the other hand, that the taking of a great quantity of meat—an excess compared with the requirements of the system—tends to increase the uric acid, though, even then, not more than in proportion to the urea.

These different facts can be advantageously applied in practice in the treatment of gravel and calculus. In such cases, there is certainly no reason why a proper quantity of animal food should not be taken; and the knowledge of this is important, seeing that many patients have been lowered in health by being kept on insufficient diet, with the idea that by these means a lessening of the excretion of uric acid would result.

Effects of Alkaline Treatment.—In the treatment of uric gravel and calculus the different alkalies and their salts play a most important part. If we give any fixed alkali in the state of a carbonate, it is, we know, absorbed, and passes through the kidneys in an unaltered form—that is, a carbonate, when taken by the mouth, appears as such in the urine, and, therefore, necessarily diminishes the acidity of that fluid, sometimes rendering it neutral or even alkaline, according to the quantity administered; so that if we give, at frequent intervals throughout the day, a quantity of the alkali equal to the neutralisation of about thirty grains of oxalic acid—the average acidity of the day's urine—we shall, as a rule, keep that fluid in a neutral state.

In considering the equivalents of the different alkaline metals whose salts are employed in medicine, we find that the neutralising power for acids of the different bases must vary considerably. The most practical method of estimating this power is to measure the different alkaline metals, in the form of their carbonates, against each other, when we find that seventy-four parts of carbonate of lithium equal eighty-four parts of carbonate of calcium (chalk or its congeners), 106 parts of carbonate of sodium, and as much as 138 parts of carbonate of potassium. It must, however, be remembered that it is not merely the neutralising power for acids of the above compounds that has to be considered in treating of uric acid disorders; we must also look at the character of the salts which result from the combination of the acid with the metal, for some urates are very much more soluble than others.

It is a matter of clinical experience that lead-impregnation powerfully disposes to the production of gout; and I can also assert, as a result of long-continued observation, that iron salts

have a considerable tendency to cause a recurrence of an attack when administered, as they often are, with a view to overcoming debility.

The difference of solubility in the alkaline urates forms a subject of great interest, for the value of a solvent may often be expressed correctly as the product of its neutralising power plus the solubility of the resulting salt. Let us take, for example, carbonate of sodium. It has been shown that its neutralising power is large compared with carbonate of potassium, 106 parts of the former doing the work of 138 parts of the latter; but, on looking at the table of solubilities, it is seen that the soda salt has less than half the solubility of the potash salt. The same remark applies to carbonate of magnesium, as, although it possesses great neutralising power, the resulting salt is very little soluble, and the very sparing solubility of the lime salt renders its employment as a solvent of uric acid undesirable.

The other properties of the alkaline salts must also be taken into consideration when they are administered as remedies. On comparing a soda salt with a corresponding potash salt, it is found that the latter is more prone to produce diuresis than the former; at the same time, there is good evidence that the alimentary canal and its appendages, especially the liver, are more influenced by soda than by potash. This is what might naturally be expected, seeing that true bile consists essentially of glycocholate and taurocholate of sodium. Magnesia salts act more or less as purgatives, and lime salts as astringents, but all act as neutralisers of acidity, and to some extent as solvents of uric acid. Gravel and calculi usually consist of free uric acid, and even the least soluble of the urates, omitting lead and iron, are much more soluble than uric acid itself.

There is one alkali—lithia—which will require some few minutes' consideration, and upon which I propose to bring forward several new observations and experiments; but, before proceeding to discuss its value, I may make some remarks on the different salts of the alkalies, some of which are more eligible for exhibition, especially in the treatment of gravel, than the carbonates.

All of us are probably aware that, if an alkaline citrate is given by the stomach, it is changed, either in the blood or kidneys, into the corresponding carbonate. The same is the case when an acetate or tartrate is administered; in fact, most of the vegetable salts are thus decomposed in the system, carbonates appearing in the urine. The establishment of this point is important, inasmuch as we can, by the use of these valuable salts, introduce into the system, through the mouth, salts which have no alkaline action on the stomach and form, often, an

important part of vegetable food, and can still produce the remote alkaline influence where it is wanted; in short, we can often give even an acid salt, grateful and useful to the stomach, and yet have the very opposite effect induced upon the urine.

I will now draw your attention to the salts of lithia, which were first introduced as remedies by myself as far back as 1859. If we look to the atomic weight of the metal lithium we find it very low, only 7. The number representing the carbonate of lithium is also small; compared with carbonate of potassium it is as 74 to 138; hence the neutralising power for acids possessed by carbonate of lithium is greater than that of carbonate of potassium in the above proportions. The acid urate of lithium requires only 220 parts of water at the body temperature to dissolve it; the corresponding potash salt requiring 500 parts, and the soda salt as much as 1130 parts, while the magnesia and lime urates take 1600 and 2800 parts respectively; so that, with respect both to neutralising power and solubility the lithia carbonate has a great advantage over the corresponding salts of potash, soda, magnesia, and lime.

In a paper in the Medico-Chirurgical Society's Transactions, 1875, on "The Solvent Treatment of Urinary Calculi," Dr. W. Roberts, of Manchester, came to the conclusion that potash carbonate dissolves uric acid more rapidly than the soda salt. This he ascertained by placing sections of uric acid calculi in phials, and causing currents of the different solutions, at blood heat, to pass over them at a regulated rate. He also found that the strength of the solution employed was of much importance, the greatest amount of solvent power being exhibited in solutions containing from forty to sixty grains of the alkaline carbonate to the imperial pint (twenty fluid ounces). Below this strength the power of the solutions gradually declined, until, with those which contained less than three grains to the pint, the solvent power scarcely exceeded that of water. On the other hand, if the strength was above sixty grains to the pint, the pieces of the calculus became encrusted with the alkaline bi-urates which were then deposited, and thus the further action of the solution was impeded. This was especially noticeable when the strength of the solutions was much above 100 grains to the pint. Even without the actual experiment with pieces of uric calculi, I think we could predict that potash would prove a more powerful solvent than soda; for we have only to glance at the table to see that urate of potassium requires, at the body temperature, only 500 parts of water to dissolve it, whereas the corresponding soda salt takes as much as 1130 parts. The soda salt certainly has an advantage in its greater neutralising power, but not sufficient to make up for the far less solubility of the resulting urate. In the paper referred to, and in his book on "Urinary

and Renal Diseases," Dr. Roberts has the following footnote: "Some experiments were also made with carbonate of lithia, which has been vaunted in recent times as a solvent for uric acid. Its power was found much inferior to that of carbonate of potash and soda. Its reputation seems to have been gained through its comparative insolubility. Only weak solutions of it could be employed." I refer to this passage because other authors have evidently been influenced by the statement; for example, Sir Henry Thompson, in his little work on "The Preventive Treatment of Calculus Disease," says: "Dr. Roberts finds carbonate of potash to be the most powerful solvent; better than soda, much better than lithia."

But I think that lithia salts are far more powerful solvents of uric acid than potash salts, while these latter are more efficacious than those of soda. We have only to take three small phials, filled with a solution of the three carbonates, of the same strength, and to put into each the same quantity of small uric calculi, the amount being such that the lithia will dissolve them. If we carry these in a warm pocket, after a short time it will be seen that all the calculi have disappeared from the lithia solution, while more than half are left undissolved by the potash, and about four-fifths by the soda solution.

I have been informed by some patients that they have been deterred from using lithia salts, although they had found them valuable, by having been told that their employment would prove injurious, owing to their caustic effects upon the renal organs. In answer to this objection I may say that I have found the action of carbonate of lithium to possess less destructive power than the corresponding salts of potash and soda upon animal tissues.

The only effect that I have ever noticed has been that, when the quantity is increased beyond a certain amount, a little tremor of the hands is produced, which passes off at once on the diminution or omission of the dose of the salt. I have known patients, of their own accord, continue the use of lithia salts for more than ten years, with the effect of entirely preventing the recurrence of the symptoms to remove which they were first prescribed, and without the production of any injurious effect. For myself, I have not the least doubt as to the value of lithia salts as therapeutic agents, and am convinced that, by their employment, depositions of uric acid in the renal organs can to a large extent be prevented. Free dilution and administration on a fasting stomach are points of much importance, which should be attended to in the administration of alkaline remedies. I have been much in the habit of using potash with lithia, in the form of the citrate or the carbonate; the former to give neutralising, the latter to increase the solvent power.

I do not believe in the value of any injections into the bladder in the treatment of vesical calculus; at the very best, the process must be most tedious; and at the present day, when the surgery of the subject has reached to such great perfection, when a calculus can often be removed completely from the bladder in a few minutes without the use of the knife, I cannot but think that the surgeon is better qualified for the treatment of such cases than the physician.—*Lancet, April* 7 *and* 21, 1883.

31.—CURATIVE EFFECT OF CHLORAL IN ALBUMINURIA.

By Thomas Wilson, M.R.C.S.Eng., Wallsend.

When Liebreich introduced chloral, he claimed for it a prominent position amongst the drugs known as hypnotics—a position which the experience of medical men has more than confirmed. Its inventor only regarded it as a sleep-producer. Little did he think of the various uses to which it might be applied. Like all new drugs, chloral has been used in various combinations for the most varied affections. No one, I think, has as yet drawn the attention of the profession to the almost marvellous effect of chloral in causing albumen to disappear from the urine, and with it the presence of an existing œdema. I am aware that no absolute results can be based upon the success which has followed the treatment of one or two cases of albuminuria; but the facts are so strong, and the results of treatment so striking, that these must be my excuse for bringing them under the notice of the profession.

Mrs. R., a delicate-looking woman, aged 40 years, was delivered of her eighth child in February last. Her children had come rather quickly; on the last occasion, it was the second within the year. During the time she was carrying her last baby there was no œdema of the legs, and nothing occurred of any importance during the period of gestation. Her last labour finished well; but, somehow or other, she never regained her strength. She suffered from night-sweats. The end of March found her suffering from cough and severe attacks of asthma. Medicine proved of little service, for dropsy set in, and the urine became albuminous. On April 13th, Dr. William Murray saw her, in consultation with me. In the early part of May, Dr. Oliver saw her with me, and at this time her condition was as follows. Both legs were very œdematous. Breathing was short and difficult; it amounted to orthopnœa, for patient could not occupy the recumbent position. The lips were markedly cyanosed, and the pulse extremely weak. There was frequent cough, but the lungs exhibited nothing very abnormal; the urine was albuminous, but no cardiac murmur was detected. From the weakened sounds of the heart, cyanosis, and difficulty

of breathing, in the absence of any marked pulmonary lesion, I was led to diagnose a dilated heart, passive congestion of kidney, and dropsy. For some time past, the patient had been taking chloral rather freely, and, to diminish it, Dr. Oliver suggested, from its tonic action, as well as its hypnotic, the use of hops. Externally and internally their employment was unsuccessful. I again resorted to the use of chloral, but only at bed-time. Fortunately, a most reliable nurse had been obtained, and, as it had been frequently noticed that the urine passed a few hours after taking chloral was lighter in colour, and contained less albumen, I got her to keep specimens of urine passed at various periods of the day, for comparison. That there might be no mistake, this work was undertaken for some days by the husband, who sat up night after night with his wife. The experiments were carefully conducted, and admirably carried through. From the regularity with which it was noticed that the urine passed after taking chloral was clearer, of lower specific gravity, and contained less albumen than that passed at other times, it was decided to test the effect of chloral by withholding its administration altogether. Until now, the patient had been improving; the albumen had greatly diminished, the œdema was disappearing, and the patient was able to be moved to the couch; but no sooner was the chloral stopped than the symptoms returned. Every medicine was now stopped, with the exception of the chloral, as it was quite apparent to Dr. Oliver and myself that this was the only remedy likely to prove of service. Daily I made a comparative examination of the urine passed at various periods, and I always nôticed that the urine which was passed after taking chloral contained a diminishing quantity of albumen. Dr. Oliver at this time made an independent examination of the urine, of which the following is a brief statement: Specimens passed after taking chloral were of average specific gravity 1016, acid, with no albumen; specimens passed at other periods contained albumen, and granular and hyaline tube-casts.

A continuation of the chloral treatment resulted in complete disappearance of albumen from the urine, and with it disappearance of the other symptoms I have mentioned. In the middle of the month of July she had so far recovered that she was able to be removed to the sea-side. At the present time she is better than she has been for many months past, and, with the exception of amenorrhœa, she is quite well. No explanation is offered as to how the chloral was followed by such beneficial results. Suffice it to say that, under its use, a lady so prostrate that she could not stand, with a dilated heart, albuminuria, and marked œdema of feet and legs—indications of a grave constitutional state—has simply been rescued from death. The chloral did not produce any apparent diuresis or diaphoresis.—*Brit. Med. Jour. Dec.* 23.

32.—CARBOHYDRATES IN THE ANIMAL SYSTEM.

By F. W. Pavy, M.D., F.R.S., &c.

I have prosecuted investigations upon the transformation of the carbohydrates within the animal system, with the result of acquiring knowledge of an altogether unexpected nature, and which I propose to detail in future communications.

Hitherto what has been observed as regards the transformation of carbohydrates by the action of ferments and chemical agents, has been a change attended with increased hydration—for example, the passage of starch into the successive forms of dextrin and maltose, and cane sugar into glucose. The issue of the researches, however, which I have been conducting recently is to demonstrate the passage of carbohydrates exactly in the opposite direction by the action of certain ferments existing within the animal system. Alike in the alimentary canal, the circulatory system, and the liver, the conditions exist by which this kind of transformation is effected. From the mucous membrane of the alimentary canal a ferment is obtainable which converts (1) glucose into a body possessing the same kind of cupric oxide reducing power as maltose; (2) cane sugar into maltose, and not glucose as formerly asserted; and (3) starch either into maltose or a dextrin of low cupric oxide reducing power.

The presence of carbonate of soda modifies the action of a maltose-forming ferment, and leads to starch passing into a dextrin of low cupric oxide reducing power instead of into maltose. The portal blood contains a ferment which possesses a maltose or a dextrin-producing power, and the contents of the portal system during digestion are charged with a notable amount of maltose sometimes, and at other times a low cupric oxide reducing dextrin. After the introduction of glucose into the circulatory system I have observed the presence of maltose. The liver also contains a ferment capable, under certain conditions, of carrying glucose into maltose, and I have further witnessed, by the same kind of action as the sugars and dextrins are moved from one to the other, the conversion of a carbohydrate into the colloidal material belonging to the animal system (glycogen), which holds the analogous position of starch in the vegetable kingdom. Evidence has likewise been supplied that by an action of the same nature as that which moves the carbohydrates from one to the other in the carbohydrate group, they are, under certain conditions, carried into a body out of the group, and thence not susceptible of being brought into glucose by the converting action of sulphuric acid; and, on the other hand, under other conditions, a substance is brought into the carbohydrate group and its nature made recognisable by the converting action of sulphuric acid and its cupric oxide reducing power.—*Lancet*, *April* 21, 1883, *p.* 680.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, AND DISEASES OF BONES, JOINTS, ETC.

33.—ANTISEPTIC SURGERY.—GERMS AND THE SPRAY.

By JOHN DUNCAN, Surgeon to the Royal Infirmary, Edinburgh.

It is curious to speculate on the fate of Listerism had our knowledge been formerly as it is now. How would these methods have been received had we known that under antiseptic dressings organisms may flourish while the wound follows what has been called an aseptic course; that all suppurations of an acute character, and some of a chronic, are attended by microbia although unexposed to the air; that the injection of an aseptic fluid may induce septicæmia; that contamination of a wound through the atmosphere is comparatively unimportant, while the means taken to prevent it are inefficient? On this last point I have made a few investigations, which I desire to lay before you.

A few years ago I began an attempt to discover the percentage which remained aseptic of cases in which major operations were performed when the skin was previously sound. I have found it by no means an easy task. It is plainly impossible to trust such tests as the odour of the discharge or the discoloration of the protective oiled silk, because they may be absent while organisms are present, and may be produced by other causes than bacteria. Search with the microscope and cultivation of the discharges in aseptic fluids appear the most reliable methods, and have been diligently put in operation. Even they, however, are not absolutely satisfactory, because they are apt to give contradictory results. In my experiments flasks became turbid and showed organisms of various kinds after impregnation with discharges in which the microscope had not detected life, and bacteria may be visible in the discharges which are difficult to cultivate. Gradually improvements were made whereby errors in these respects were to a certain extent eliminated. The various methods of staining came to be at our disposal, and the use of different cultivating fluids was practised. The results did not even then accurately correspond, but at least it was possible, from first to last, to err on the side of caution, and accept as truly septic only those cases in which no

doubt could possibly exist. I was astonished, during the research, to find that in more than half the cases, most of which I had hitherto regarded as aseptic, organisms could be at one time or other discovered. My first idea was, of course, that this arose from defective skill in the management of antiseptics. But in any case it was a matter of importance. If the error were manipulative, then the difficulty of manipulation was so great as to make the treatment unsuited for common use; if not, there was a leak in the Listerian hypothesis. Precautions in the treatment were redoubled, and, to check my own results, I obtained from my colleagues dressings from cases which they believed aseptic. The percentage was somewhat reduced, but yet I found that about forty per cent. was the proportion which I could not by any means diminish, and that, in short, almost every case which was not after the first day or two absolutely dry, showed organisms in the fluid exuded.

It was impossible to continue in the belief that the fault lay solely, or even mainly, in defect of manipulative skill. Did they come, then, from within the body or from without? Was it, or was it not, a result of imperfection in the methods of Listerism?

And first, do they come from within? There are various experiments and observations which bear upon this point. That organisms of various kinds easily find their way into the blood by other channels than by palpable wounds may be held as certain. We have in evidence the whole class of zymotic disease, and more especially those in which the presence of microzymes has been directly demonstrated. That from the blood they may pass into and develop dangerous energy in the tissues, the published cases of endocarditis and osteomyelitis sufficiently testify.

The curious experiments in bistournage are doubtless capable of several explanations; but if they may be held to indicate the rarity with which certain organisms are present in quantity in the blood, they indicate, also, their power for evil and the certainty with which, when present in the circulation, they attack tissues devitalized by injury.

Dr. Burdon Sanderson has asserted that by injection into the peritoneum of aseptic fluid in sufficient quantity bacteria of virulent quality may be there engendered, an experiment which suggests that a certain proportion between the number present in the blood and the amount of local irritation is necessary to ensure their development.

Lastly, Dr. Ogston's observations tend to confirm this view. He found in a long series of acute abscesses the invariable presence of chains or groups of micrococci. I have repeated his observations by his methods in most of the abscesses which

have come under my care since his paper was published. In three of the acute abscesses, a bubo and two whitlows, I was unable to find organisms; in the others they were present. All contained micrococci, several contained rod-like bacteria, and one a number of long jointed rods. I have not found them so commonly in chronic abscesses. In a few, however, they were undoubtedly to be seen. Thus, in two strumous glandular abscesses of the neck and in one of the axilla I found multitudes of micrococci. Chronic abscess is probably more frequent in the abdomen than elsewhere. Except those connected with caries of the spine, in which I have only once met with them, organisms, generally bacteria, as well as micrococci, are constantly to be found in abdominal abscesses. Two of those which I examined were nephritic abscesses, one being due to the presence of a calculus in the kidney, two were probably from chronic perityphlitis, the others of undetermined origin. It is, of course, possible that in some of these cases contamination may have spread from the neighbouring digestive or urinary tract, and that in others the organisms arrived by the lymph path, but this explanation cannot apply to many. It is impossible to avoid the conclusion that if the abscess be the effect of their presence, their entrance into the blood is comparatively common; if it be the cause, their great frequency in acute abscess would indicate either their constant presence in the circulating fluid, or that very slight disturbance of the general economy may afford the necessary conditions.

It appears certain, then, that we have here what I have called a leak in the antiseptic system. It is certain that organisms are frequently found in discharges apparently aseptic. It is proved that similar organisms easily enter the economy otherwise than by a wound. It is proved that organisms thus entering may produce local effects of varying degrees of severity, and are prone to attack injured textures. It is proved that they almost invariably attend acute inflammation. Is the conclusion not warranted that they sometimes present themselves by this means in surgical injuries, and that the severity of the resultant effects will be determined by the same causes as influence them under other circumstances? What these causes are we have yet in some measure to determine. Some of them, at least, we know; others we can only conjecture. We know that moisture and warmth favour the multiplication of these organisms, and that dryness and drainage are inimical to them. We know that the healthy textures may absorb a certain quantity of fluid, may destroy a certain number of microbia, but that beyond a certain point their germicide power does not extend. We believe that very various properties belong to different germs, whether from innate generic and specific attributes or as

the results of cultivation natural or artificial. The results of moisture, of warmth, of drainage, of tissue health, of differing potencies, apply with equal force to germs which come from within and from without. My observations as to the presence of bacteria in wounds suggest to me that they are less harmful when they occur under antiseptic dressings than otherwise. May it be that their passage through their blood often deprives them of a portion of their virulence, or is it that the forms harmless to the blood are so also to the tissues more or less? I excised a knee-joint for anchylosis in complete flexion. The wound healed in less than three weeks, and after the first day or two was dressed only once a week, while the temperature never rose above 99°. In the scanty watery discharge abundant rod-like bacteria were invariably found, although absolutely in every other respect it followed an "aseptic course." And what adds to the striking character of the case is, that when, six weeks afterwards, in the same patient, for the same cause, I excised the other knee, the result was the same. That is merely one example out of many, and I have no hesitation in believing that had I allowed these wounds, while exuding fluid, to become contaminated from without, the result would have been modified to a greater or less extent.

Is, then, the difference between a compound and a simple fracture, an open and a closed bruise, to be accounted for solely by the presence of bacteria in the one and not in the other? A patient was admitted to the Infirmary who had ruptured, by sudden strain, some of the adductor fibres in his thigh. In the extravasated blood suppuration occurred, and when I opened the abscess I found many micrococci. Why does this suppuration not more often occur? The suggestions given above may partly explain; and it may be that although micrococci come to many, they find blood-clot pure and simple an unfavourable soil. Much has been said of the evils of tension such as existed in the case referred to. Tension does harm, undoubtedly, but not so much alone and in itself as by the nature of the exudation which causes it or to which it may give rise. The invariable tension of a bruise by blood-clot causes little harm. Tension by bloody serum, in developing bacteria from within or from without, may result in suppuration and septicæmia. But it would be impossible within reasonable limits to pursue these thoughts. They are sufficient to enforce the practical importance of the general condition of your patient prior to an operation, and the necessity for precautions against the development of germs coming from within.

But it must be admitted that contamination comes also from without, and to determine the question to what extent the organisms we find in wounds are due to one or other cause, the

efficacy of external preventive measures must be ascertained. Can we, independently, I mean, of the necessary failures arising from human fallibility, by our present modes of antiseptic dressing, insure the exclusion of external septic influences? And, first, are the materials with which we cover our wounds able to exclude organisms? It is quite certain that with flasks containing sterilized but putrescible fluids many materials may be used for that purpose with success. It is sufficient to cover with cotton-wool, gauze, or almost any thick or impenetrable material, which has first itself been sterilized. I have found one of the most convenient coverings in experiments with such flasks to be a layer of ordinary pink mackintosh, because it diminishes evaporation. It is not infallible. If you hold it up to the light, you will see that the waterproofing is often imperfect, that there are minute holes. Sometimes, presumably from this cause, it fails. Thus on one occasion I prepared six large beakers of Darby's fluid meat, and, having made certain experiments with them, set aside four which remained aseptic after three days' incubation. At the end of eleven weeks a delicate globe, like fine wool, began to form in the centre of the fluid in one of them, ten days later in a second, and gradually the whole contents became mouldy, while the other two remained unchanged for several weeks longer, and were at length used for other purposes. The macintosh may fail, then, but it does so very, very rarely; and I have frequently laid the flasks and beakers on their side for many hours, with the fluid in contact with the macintosh, without the contents becoming septic. But it is necessary that there be no putrescible channel leading from the outer air to the inner liquid. Such a channel speedily leads to putrescence of the liquid. Thus a flask containing Darby's fluid beef in aseptic solution was covered with macintosh, incubated, and kept a week. It was then laid on its side for a few minutes. The macintosh was made so tight that only a very little moisture escaped. Again incubated, it still kept clear. But after being kept on its side for twenty-four hours, a hardly perceptible outflow being thus kept up, it rapidly became cloudy on incubation.

Some interesting corollaries follow from these facts. Among them, for instance, this, that indiarubber and other materials of a like character, when thoroughly purified, may be used, with proper care, as in themselves a protection from sepsis; and that the macintosh or gutta-percha tissue are sufficiently safe as the outer layer of a dressing. It is evident, then, that with any of the ordinary forms of antiseptic dressing the only fear of contamination arises from the discharge or other putrescible fluid communicating with the outer world at the margin for too long a period. That this is an occasional source

of sepsis, especially when dressings remain long unchanged, is certain and generally recognised. Mr. Watson Cheyne's observations indicate, in his opinion, that this is apt to happen with micrococci when an ordinary gauze dressing has been kept on a very few days. And it must be remembered that the ordinary secretions of the skin, confined by impermeable material, may form the necessary channel as well as pus, a strong argument in favour of dry and absorbent dressings.

These observations of Mr. Watson Cheyne's, which may be suggestively compared with Dr. Ogston's on micrococci in abscess, receive also an explanation in a little experiment which I made with carbolized gauze. I covered the floor of the incubator with sixteen layers of antiseptic gauze, and exposed it for an hour to a temperature of 98°. I introduced a beaker of aseptic fluid, and left it exposed for twelve hours. It was then covered with macintosh, and remained clear till the fourth day, when it was again uncovered for twelve hours. It now became putrid, as did also beakers introduced on the fifth and sixth days. It is clear, therefore, that at least from the fourth day onwards the ordinary carbolized gauze maintains a wound aseptic, not because it contains an antiseptic, but because of the filtering material of which it is composed, and in which respect it is much excelled by dry cotton-wool and other dressings.

It is probable, then, that a certain proportion of the cases in which we found microbia may be accounted for by delays in dressing and by the nature of the material employed. There is here a tendency to failure, but one which can be guarded against by proper precautions. Any antiseptic dressing in common use may be rendered safe and effective.

But, secondly, are the precautions which we take when wounds are uncovered, as at the time of operation or the times of dressing, sufficient to protect them? Is the spray, for example, efficient? When engaged in testing wounds by cultivation experiments, I was in the habit of employing flasks for purposes of control. Two flasks, in every respect similar, were brought to the wound, opened and closed simultaneously under the spray, while one was inoculated with discharge and the other not. In two cases both flasks went wrong, and in one the flask which had not been inoculated became septic, while the other remained pure. I began to doubt the spray. *A priori*, one would be strongly inclined to believe that the rapid passage of septic material through an attenuated spray would not be sufficient to neutralise its malevolent properties; and that the passage might often be extremely rapid is very likely. I instituted, therefore, a series of experiments with aseptic solutions. It is hardly necessary to premise that every precaution was taken, in the way of washing with strong antiseptic solutions,

not only hands and instruments, but also the coverings and external parts of the vessels before opening and closing them, so as to prevent contamination otherwise than through the air. The following was the method adopted:—A weak solution of Darby's fluid meat was rendered aseptic by boiling, and incubated. Two similar vessels containing it were opened simultaneously on the same table, one under the spray and the other without the spray, and were kept open for the same time. They were then reclosed and incubated at a temperature varying little from 98°.

It appeared probable that the fall into the fluid of carbolic solution from the spray might render it an unsuitable soil for germination. Many of the earlier experiments were therefore made with coverings, such as bell jars or sloping or horizontal plates of glass, suspended over the flasks or beakers. It was found, however, that this precaution was unnecessary, because uncovered vessels over which the spray had been playing for long periods were rendered muddy and putrescent with as great facility as others. I at first used beakers an inch and a quarter wide and two inches in depth. We found that at least twenty minutes' boiling was necessary to render them aseptic, and that in that time at least half the fluid had evaporated. We began with an exposure of one minute, and gradually increased the duration to forty-five minutes, with the following result:—Thirty-eight flasks were exposed under the spray, of which seven became putrescent; thirty-seven (one being spilled) were exposed without the spray, of which nine became putrescent. This is, of course, an unfavourable result for the spray. It might be, however, that some of the failures on both sides, comparatively few as they were, arose from manipulative defect. Thus the incubation period in some of them we found was not sufficiently long, because we subsequently met with one beaker in which cloudiness did not arise till it had been kept for four days in the incubator. Now we had been content with two days' incubation, and in some with only one. In several cases the same beakers had been used twice, and although the two sides were in all cases kept strictly parallel, it was unsafe to draw absolute deductions. It would only become a satisfactory experiment when we reached the period of exposure when putrefaction invariably ensued if the spray were not used. But with these small beakers this could hardly be reached without introducing other errors from the absence of personal supervision. Thus one of them was exposed without spray for periods of forty-five, fifty, and fifteen minutes on successive days without losing its perfect clearness. It was necessary to use larger tumblers. Beakers two inches in diameter and three inches in depth were exposed for periods of twenty, twenty-

five, and thirty minutes. Of five under the spray one became septic; of four without the spray one also putrified.

Seven beakers $2\frac{3}{4}$ inches in diameter were exposed for the same periods with and without the spray. On both sides two remained clear and five became cloudy. We were approaching now a more accurate period of exposure. Beakers $3\frac{1}{4}$ inches in diameter were procured. But we were now confronted with a difficulty which had been gradually increasing with the size of the beaker. We found it almost impossible by prolonged boiling to render the contents of a vessel of this size aseptic without causing such evaporation as to reduce the quantity of fluid and increase its concentration too much for any useful purpose. By many experiments we came to the conclusion that this was due to the quantity of fluid, and not to the shape of the vessel or its proportion to the amount of contained liquid. I am unable to explain this curious fact, but it is indubitable that in proportion to the size of the beaker we required from twenty to ninety minutes to attain an aseptic state with fair certainty. We overcame the difficulty by Mr. Tyndall's plan of interrupted boiling, and we found, after many attempts, that we could insure success with beakers of any size by boiling five times, for one minute each time, at intervals of twelve hours.

Beakers of $3\frac{1}{4}$ inches were now, therefore, tried. It was found that up to twenty minutes' exposure occasionally they remained clear. Beyond that limit I have not succeeded in maintaining a single example unclouded, nine having been used on each side, four with twenty-five and five with thirty minutes' exposure.

I do not know that it will be necessary to multiply these experiments. I am unable to see where error can creep in. They seem to me definitely to prove that, so far as the destruction of floating germs in the air is concerned, the spray is perfectly ineffectual. It may be a convenient method of throwing a solution of carbolic acid of the strength of one to forty upon a wound. It may thus be useful in open wounds, but it can be of little value in such operations as those which deal with deep cavities. Nay, I should think it likely to prove more injurious than beneficial in such cases. If you make the spray play across a beam of sunlight, you can see the floating dust in clouds rushing towards and being whirled along with it, so that one can hardly doubt that the margin of the spray is the most dangerous position in which a wound can be placed, and that a slight deflection from a current of air may result in the entrance of this floating matter to recesses where the solution deposited by the spray may never reach.

But these same experiments also show that the risk of contamination from the air has been greatly exaggerated. Putrescence invariably follows a sufficient exposure of putrescible fluid to the air; but the germs which float about cannot be extremely numerous if a surface of an inch and a quarter may be exposed for nearly two hours without effect, and if to be reasonably certain of putrescence a surface of three inches and a quarter requires an exposure of twenty minutes.

The conclusion evidently is, that in the matter of wounds our prime object ought to be the attainment of the most absolute purity in all substances brought into actual contact with them, while the rare contamination which may come through the air is guarded against by washing with antiseptic solutions or by giving the germicide powers of the living textures a fair opportunity of coming into play. This conclusion is amply confirmed by the results of those who have not used the spray. The success of Keith and Lawson Tait in ovariotomy, and the statistics of Spence and Callender, not unrivalled by many others, in general surgery, are thus explained.

If, then, it be taken for granted that the spray is ineffective, other points for investigation are opened up. I hope to lay before you shortly the results of some researches in which I am at present engaged as to the germicide property of the vapours thrown off at different temperatures by various antiseptics. The volatility of the antiseptic used in a dressing has been considered of some importance. I have already pointed out that this is not sufficient to outweigh other considerations, and that the efficiency of a dressing depends rather on its material and on the ability of the antiseptic so to mingle with the fluids that they become unsuitable for the development of microzymes. I have ascertained, however, that the vapours of carbolic acid and eucalyptus, among others, are germicide at the temperature of the body.

It is possible that in this direction we may discover a substitute for the spray, as, for example, by rendering aseptic the air of the operating chamber. It has already been proposed by Mr. M'Gill to pump upon the wound the vapour of eucalyptus. The following experiments, however, indicate that this is not sufficient.

1. Three ounces of eucalyptol were placed in a shallow dish on the floor of an incubator. This was closed and the temperature kept at 100° F. for an hour. Three three-inch beakers containing aseptic Darby's solution were then introduced, uncovered, and left in the incubator at the same temperature for two days. The beakers were then covered and farther incubated, but all remained perfectly clear.

2. If the experiment be repeated, with this variation, that

the eucalyptol is exposed in the incubator for five minutes only before the introduction of the beakers, then all become turbid on incubation, even though they have been exposed for only twenty minutes.

3. Six fluid ounces of eucalyptol was placed on the floor of the incubator and kept at a temperature of 60° F. for one hour before the introduction of the beakers, which were left exposed for twenty-four hours. All, three in number, became putrid.

These experiments require, of course, repetition and variation to arrive at exact knowledge. But we can see that the elements of time and temperature are of importance, and that a mere draught of eucalyptol vapour is not enough. It is necessary that it be given off in a certain volume, under a certain heat, and that the air be exposed to it for a certain time.

In this direction, however, farther investigation is required, both as to the efficacy and innocuity of the various antiseptic vapours.—*Edinburgh Medical Journal*, *March* 1883, *p.* 778.

34.—ON THE TREATMENT OF RECENT WOUNDS.

By Roderick Maclaren, M.D., Senior Surgeon to the Cumberland Infirmary.

No one method of treating wounds is likely to give the best attainable results under all circumstances and with every variety; and, when a man sets forth that one treatment best promotes the healing of every such injury, I think that he is either generalising on imperfect observation, or allowing enthusiasm to warp his judgment. I purpose considering wounds under their ordinary divisions of incised, contused, and punctured.

First, as to incised wounds: for wounds of small or medium size, not involving bones, cavities, or the sheaths of tendons, I have seen the best results from the most absolutely simple treatment. 1. The first thing is careful cleansing of the wound, and removal of any foreign substances, if such be present. For this purpose, I prefer carbolic lotion (1 to 20); boracic lotion (saturated); or even simple dry rag or other convenient material. I am convinced that the water in common use is often a medium which brings deleterious matter into contact with a wound, and, for that reason, to be avoided. 2. Next to this follows suppression of hemorrhage; and the more thoroughly this is done, the better is the chance of primary union; indeed, the ability to completely control it is one of the limits to this simple treatment. Ligature of vessels with catgut, and torsion, are the simplest means at our command. Sometimes, specially if a wound be over bone, a pad of absorbent cotton-wool, applied for twenty-four hours, will stop oozing. 3. The next stage is the bringing of the sides of the wound into complete contact.

For this purpose, I am in the habit of employing one of two methods—suture or pressure. The sutures I prefer are silver wire, hair, or common sewing-needles. I believe it to be an advantage to use very fine sutures; and, for small wounds, hair answers most admirably; it is very strong, unirritating, and easily procured. A very excellent way of closing scalp-wounds is to push a sewing needle through the two edges, and bring them together with a thread in figure-of-eight. In using sutures of all kinds, it is of great consequence to pass them through the tissues beyond the wound, whenever this can be done. Thus, with hare-lip, after paring, I pass needles right through the whole thickness of the lip and mucous membrane, keeping them (the needles) entirely from contact with the wound. I have sometimes seen suppuration commence at a suture, and spread from it, breaking up a wound which was promising to heal well. This risk can be avoided by keeping the suture away from the cut surface. Occasionally, a wound is so placed that a pad will close it throughout. Absorbent cotton-wool is the nicest material for this purpose. If it come near the edges, it is an advantage to have it impregnated with an antiseptic. When a wound of the class I am speaking of is efficiently closed, and all hemorrhage has stopped, I look on any dressing not only as superfluous, but generally as injurious. Simple exposure to the air, keeping the wound dry, cool, and, above all, at rest, are all that is needed. We all know that heat and moisture promote putrefaction; and they should, therefore, be avoided. Sticking plaster is, of all known ways of keeping a wound together, the worst, almost invariably producing suppuration of the parts with which it is in contact. The only departure from the above way of treating moderate-sized flesh-wounds is where the surroundings are unhealthy, and especially in hospital, where the presence of suppurating surfaces and various ailments causes special risk of infection; then the complete Listerian dressing is the proper one to adopt.

I will next speak of large flesh-wounds, wounds involving joints and other cavities, and wounds involving bone, such as amputations. These may be all classed together, for one treatment is adapted to all. I am not aware of any treatment which will, in the majority of cases of this class, secure primary union; nor do I think it is likely that any such will ever be discovered. No doubt it occasionally occurs under various methods of dressing; but where there is considerable oozing, which cannot be completely stopped, where muscle is no longer in contact with muscle, tendon with tendon, or subcutaneous fat with fat, but each in apposition to tissue of another kind, it does not seem likely that primary union will ever be the rule. In all cases of this class, I regard asepticism, in its most exact signification,

as involving least risk to life, and as most favouring local cure, of any method of treatment with which we are acquainted. No method of maintaining asepticism equals the carbolic gauze dressing, with all the adjuncts of spray, carbolic lotion, &c. I have tried, or seen tried, a great variety of other dressings avowedly antiseptic; but no other as yet seems worthy of holding a permanent place. I have no doubt that a better will be discovered. Its disadvantages are obvious enough. It is troublesome, cumbrous, somewhat irritant, and, above all, it keeps the wound sodden; and this unquestionably is not the most favourable condition for healing. But its certainty—the fact that it *will* do what we intend unfailingly, where the conditions are at our command, and we make no mistakes—places it above everything else yet brought forward. The drainage-tube is too essential a point of this dressing to be passed over without a word; it provides a ready channel for the escape of discharges, and prevents distension. Drainage should be limited to as few lines as possible, so that the surfaces may be kept thoroughly in contact.

Only one set of incised wounds remains, where the wounds are connected with suppuration already existing, as in opening an abscess, excising a suppurating joint, or removing dead bone. Unless the whole surface which discharged pus can be removed, such wounds may be expected to continue suppurating. In these, also, complete asepticism should be secured whenever possible; but, in a great many, from the impossibility of reaching every corner where putrefaction dwells, it cannot be attained, and I believe we then do best by using some efficient unirritating antiseptic, such as carbolised oil (one to twelve), applied on lint, and frequently renewed. In this class, I find myself yearly using the drainage-tube less and less, preferring, in a great majority of cases, to make a large wound, and leave it fully open. If the case be one where there is a reasonable probability of securing complete freedom from putrefaction, I put a plug of carbolised gauze in the wound, and an ordinary gauze dressing over it. If it seem hopeless to expect an antiseptic condition, I plug the wound with lint, soaked in carbolised oil, and have it frequently changed. Excisions of joints, as of the elbow and hip, I now treat in this way, and have got better results than when sutures were used. There is no possibility of discharge being retained; the wound heals uniformly from the bottom, no pockets of pus or sinuses remain; I believe healing is more rapid. I showed you at last meeting a case of excision of the elbow for suppuration in the joint and sinuses communicating externally, which was absolutely healed in five weeks and four days after the operation. These remarks apply also to cases in which a cavity, that the tissues will not fill, remains

after an operation. It is better to leave such a cavity open, than to shut it up by pulling skin over it.

I now come to contused wounds. When these approximate the characters of incised wounds, as so often happens with the scalp, the edges being sharp and clean, with little damage to the tissues around, they should be treated as incised wounds. The results are often surprisingly good. Large, irregular, branching wounds, caused by a blow from some blunt instrument, or a fall, often heal without any formation of pus. If these are at one end of the scale of contused wounds, at the other are cases where there is no doubt that the vitality of the injured part is for ever destroyed, and that amputation is our only resource. As this brings them into the class of incised wounds, they call for no further remark. But there is an intermediate class, where the amount of injury is doubtful, and where only time can show whether the tissues can live or not—cases, I may say, often of great responsibility and great anxiety. Such injuries, if seen early, should have antiseptic measures put in force at once. To be successful, they should be done very early, as everything in the condition of the wound favours the speedy advent of putrefaction. Very many cases so treated will well repay the trouble. I have seen apparently hopelessly damaged tissue retain its vitality, and I have seen sloughs slowly absorbed without constitutional disturbance or infective sequelæ. When a contused wound of this kind becomes putrid, I generally have it dressed with carbolised oil or charcoal poultices until the sloughs are mapped out, and then remove these as speedily as possible. I do not think too much importance can be attached to this, for dead and putrefying tissue forms a terribly active centre of constitutional contamination. Continuous irrigation with carbolic lotion or Condy's fluid, gives often good results with this kind of injury, specially where efficient treatment has not been instituted at once, and where widespread putrid suppuration is going on. It washes away the discharge as it is formed, and by its mechanical, as well as by its antiseptic action, it tends to keep such a wound sweet.

Punctured wounds do not call for much comment. They bear a small proportion to the other classes in civil practice. Their importance depends very much on the tissues or organs they involve, and it would be beyond the scope of these remarks to go into details. Small punctured wounds heal readily with rest alone. For medium-sized ones, an ice-bag is a useful application, the larger ones should be treated antiseptically.

In conclusion, I purpose making a few remarks on the principles and objects we have to keep in view in treating recent wounds. First in importance is the avoidance of constitu-

tional infection; and, second, the speedy healing of the wound, and the restoration of the part to usefulness. These are not synonymous for a treatment which, if successful, results in speedy healing, but may, nevertheless, involve great risk of general infection in the event of its failure, such as the dry treatment of large wounds. I think it is our duty always to keep before us the gravity of the constitutional risk, and to consider first what reduces this to its lowest point, and to regard local recovery as an important but still secondary matter. If we inject a putrid fluid into an animal, we can get exactly the disease which used to afflict our surgical wards, and the evidence is very conclusive that the poison which produces grave general risk, is inseparably connected with putrefaction. Anyone who goes over the experiments of Pasteur, of Lister, of Roberts, or of Tyndall, can, I think, come to only one conclusion, that putrefaction invariably originates in infection from without. But it needs a suitable soil, a certain amount of moisture and heat. Various substances are prone to it in various degrees, living tissues much less than dead ones, healthy much less than unhealthy, thick fluids much less than thin. We may, therefore, endeavour to carry out an antiseptic treatment from two opposite directions. We may either make the soil a very unsuitable one, as in the dry treatment, or we may prevent the access of septic material, as by the Listerian dressing. Experience and science alike tell us that, while we look after the wounds, we must not forget the body in which they are, and that pure air, good food, and careful nursing are not less essential than lotions, and sutures, and dressings.—*British Medical Journal, Dec.* 23, 1882, *p.* 1244.

35.—ON THE "DRY DRESSING" TREATMENT OF WOUNDS.

By Sampson Gamgee, Esq., F.R.S.E., Consulting Surgeon to the Queen's Hospital, Birmingham.

Generalisations are proverbially difficult in a science and practice like that of surgery. However sound be their foundation, however close the reasonings by which they are arrived at, their success in particular cases depends on the judgment, skill, and care with which they are applied. To the reservations already made I must add something on "dry dressing," which, unqualified, is a very misleading designation of this plan of treatment. It is certainly entitled to be called "dry dressing," inasmuch as water is not used, and even astringent or antiputrescent lotions very sparingly so; but success demands attention to all the essentials of the physiological treatment of surgical injuries—immobility, position, and pressure, drainage and infrequent dressing, pure and non-putrescent materials;

gentle, patient, and skilled manipulation; intelligent and unceasing watchfulness of constitutional states.

Fresh wounds without loss of substance are particularly suited for the plan of treatment here recommended. They should be put up without water, the edges accurately in contact; always bearing in mind the necessity of providing for drainage outwards of any effused fluid. Under absorbent pads and elastic pressure, with absolute rest and attention to position, the vast majority of fresh wounds heal rapidly, solidly, and painlessly. When the dressing is changed, which it should only be infrequently, no water should be employed; but if there be any discharge and necessity for cleaning, this can best be done with a pledget of dry lint or of absorbent gauze and cotton; all manipulations to be of the lightest. Such dry dressing simulates the natural scabbing process, but is really more perfect. Wounds of many inches in length heal so directly and perfectly under dry dressing and elastic pressure, that in the course of a few days it is often difficult to detect the fine linear scar on the dry and shrivelled skin. If a fresh wound be attended with loss of substance, some boroglycerine should be poured on the part before application; it prevents too close adhesiveness, and possible bleeding, when the dressing is removed, and has the further advantage of preventing decomposition.

The necessary employment of sutures and adhesive plasters, according to requirements, need not be dwelt upon, and I shall only briefly remark that instead of, or in addition to, such bonds of union, I frequently employ styptic colloid, compound tincture of benzoin, or collodion.

In wounds with large loss of substance, if healing be slow, action may profitably be stimulated by a variety of the well-known astringent applications in ointment or lotion, than which I do not know a better than the old red lotion, with a liberal addition of glycerine. Position, rest, and pressure remain cardinal indications, poultices and water prohibited. By this I mean stagnant water in the shape of water dressing, which is nearly as potent as a poultice in promoting suppuration and decomposition. It is otherwise with cold-water irrigation, which is consistent with, nay may be made conducive to, perfect drainage, and by its astringent and sedative action produces effects very similar to those of rest and pressure. Cold irrigation is not easy to apply continuously comfortably, and one of its great advantages, the low temperature, may be secured by ice bags.

I hope I have made it clear that while the absence of water is a prominent feature of the dry dressing method, an essential is the maintenance of immovable apposition under elastic pres-

sure, whereby the dynamics of the circulation are so controlled that the part is only allowed blood enough to nourish it. Irritation, the great cause of stasis and effusion, is reduced to a minimum, and the part is maintained in a state the nearest approaching to inaction and dryness. In direct proportion the material and the possibilities of decomposition are averted.

Contused and inflamed wounds likewise afford conclusive evidence of the soundness and general applicability of the principles and method just related. The dressing which I hold in my hand was removed from one of the *employés* in an iron warehouse. He was moving some pigs of iron, when one, weighing a little over a hundredweight, fell on his right foot. I saw the case very shortly afterwards, and found the foot very much swollen, its bony outline obliterated, the skin bluish and shining, with a star-shaped wound on the centre of the instep. Having satisfied myself that no foreign body was present, I dried the wound and placed over the dorsum of the foot this fold of lint, well soaked with compound tincture of benzoin, over it this large pad of absorbent gauze and cotton, and then a compressive bandage from the roots of the toes to the middle of the leg. I enjoined my patient to keep perfectly quiet, lying during the day with his head at the foot of a sofa and the injured foot over its head. I did not remove the dressing until the eighth day, when the wound was healed, the outline of the limb perfect, and though the skin was mottled, as from a bruise, up to the middle of the leg, it was cool and painless.

You see how the blood had penetrated, though in small quantity, through the dressings, and dried on the outside. The tincture of benzoin had acted as a coagulant and anti-putrescent, and drying into the lint served the purposes of a mould. Its styptic property was assisted by pressure and position, under which the effusion was absorbed; the part shrank, and the wound healed without any further interference. This result, a typical one of the method, was not a simple consequence of a dry application, but due to a variety of causes which combined in controlling the circulation and promoting reparative action in accordance with demonstrably true principles of animal physics.—*Lancet, Nov.* 25, 1882, *p.* 885.

36.—ON THE TREATMENT OF HIP-JOINT DISEASE.

By S. D. Clippingdale, M.D., F.R.C.S.

Any constitutional complication of the disease may require a constitutional remedy, but the constitutional treatment of the joint disease itself is identical with that which is required to keep the body of a healthy individual in a state of health, and consists in attendance to food, air, and hygienic requirements generally.

Any part of the body that is inflamed requires rest for its recovery. This is especially so in the case of an inflamed joint. If an inflamed part be not healed by rest, then, in order to save life and lessen suffering, its removal becomes advisable; this is what is necessary and practised in the case of an inflamed hip-joint.

The treatment, therefore, of a case of morbus coxæ, consists, firstly, of rest, and if this be not effectual, then excision of the joint demands consideration. Various opinions have been entertained by equally competent authorities as to the best method of obtaining rest; as to whether fixation alone of the joint is sufficient, or whether fixation should be combined with extension, and whether rest can and should be associated with movement of the joint. These views, and the methods of carrying them into practice can be conveniently described under three heads. These are 1, simple rest; 2, rest with extension; and 3, rest with extension and motion.

1. *Simple rest* to the joint may be secured by enclosing the limb in a casing of such material as gutta-percha, leather, wire-gauze, starch, or plaster-of-Paris. It may also be obtained by the application of the long splint of Désault or Liston; or by the splint bearing the name of its inventor, Mr. Thomas, of Liverpool. This valuable splint is so commonly used that a description of it here seems unnecessary. A detailed account of it, however, such as to convey a thorough knowledge of its mechanism, and enable the practitioner to have one made "with no more mechanical aid than can be obtained from the village blacksmith and saddler," will be found in Mr. Thomas's work on "Diseases of the Hip, Knee, and Ankle."

With regard to the use of gutta-percha, plaster-of-Paris, &c., it may be stated that this method is effectual, but that the material is not always at hand, and requires experience in its application, and it is to be noticed that these appliances hide the affected part from view, a serious defect. The long splint is far more serviceable, and is in constant use in the hospitals of London. Its defect is, however, that it does not render the joint immovable in every position of the body; for, passing along the side of the body only, flexion of the limb can take place when the patient is turned over for nursing purposes. This objection does not apply to Thomas's splint, hence its peculiar value. Thomas's splint has been said to produce more wasting of the limb than any other. This is difficult to understand, for the wasting depends upon the amount of inaction, and not upon the form of splint used.

Of these various methods of securing rest, I would venture, therefore, to record my testimony in favour of Thomas's splint, although I duly acknowledge the great value of the long splint,

but its failure to prevent flexion of the joint in all positions of the body, I consider a serious objection to its use.

To ensure the most perfect rest, the patient should be kept in bed, and not only kept in bed, but secured to it. It is advisable to confine the sound limb as well as the affected one. This is done at some hospitals by the use of the "box-splint," or it may be effected by Thomas's double splint. It is of importance to attend to the structure of the bed, and other details of treatment. The success of a case often depends upon attention to these, apparently trivial, matters. The bed should be firm and flat, the head kept low, and the trunk immovable. Possibly, I cannot do better than describe the excellent plan carried out at the Alexandra Hospital for the treatment of Hip Disease in Children; there, the bed consists of a single plank, upon which lies a hard mattress; the pillow is small, just large enough for the head, about a foot square, and flat. To secure the patient's body to the bed, an armlet is passed over each shoulder, the two armlets are fastened behind to a strap which passes across the back and is secured to the bed underneath. In front, the armlets are also connected by a band across the chest. This arrangement renders the patient immovable in bed, and doubtless contributes materially to the good results which seem to follow the treatment at the Alexandra Hospital.

2. *Rest with Extension.*—According to Sayre, this principle was first recognised and carried into practice by Dr. Harris, of Philadelphia, in 1825. Dr. Harris seems to have used the long splint with extending and counter-extending bands. The mode in general use, and deservedly the favourite mode, is that by weight and pulley. In this method the trunk is fixed in some way so as to produce counter-extension, and extension is made by a weight attached to the limb. The weight and pulley seem to have been first employed by Sir Benjamin Brodie in St. George's Hospital. His account of it is to be found in the third edition of his work upon diseases of the joints. In the application of the weight and pulley it is to be borne in mind that, by a law of mechanics, the weight is diminished by one-half by the passage of the cord over the pulley, so that if the weight be four pounds, the actual tension upon the leg is two pounds. Care should be taken to use no unnecessary amount of weight; only sufficient should be used to overcome the resistance of the muscles.

Instead of employing the weight and pulley, Mr. Barwell uses a splint designed by himself. Briefly, this apparatus may be described as consisting of a long splint with two pullies, one at each end. The cords from these pullies are connected together by an accumulator placed upon the outer side of the

splint so that by this mechanism, extension and counter-extension are both manifestations of the same force.

3. *Rest with Extension and Motion.*—In this method of treatment the object aimed at is motion of the joint without friction of its surfaces. This plan originated and is principally carried out in the United States of America. The idea seems to have been conceived by Dr. Henry G. Davis, of New York, and first carried into effect by him in the year 1855. The apparatus he used is known as "Davis's splint," but there have been important modifications of it by Sayre, Taylor, Washbourn, and J. C. Hutchinson, all American surgeons. This method, which may with propriety be called the "American method," finds most favour in the country of its birth. Either from want of faith, or want of experience, the plan has never been adopted, although it has been tried repeatedly, in this country. The surgeons who practise this method write of it as follows:—Davis, the originator, says it occurred to him that if the principle of continued elastic extension "could be so applied as to allow the patient to walk and ride, and take out-door exercise, we should not only add to his comfort and happiness, but it would aid in invigorating the system, thereby enabling it not only to resist the disease, but to repair the injury that might already have been inflicted." And again he says:—"This we consider the essential element in the treatment of the disease under consideration, viz., motion without friction, for no part of the anatomy requires motion to maintain its integrity more than the joints." C. F. Taylor observes "it is pressure or motion under pressure which is the destructive agent in disease of the hip-joint . . . . and motion in the joint without pressure is not only not injurious, but is highly beneficial." Sayre writes:—"Motion is as essential in retaining a healthy condition of the structures about a joint, as light is essential in retaining a healthy condition of the eye."

All this is true. It is perfectly true that exercise is necessary to maintain the functions of a joint, but it must be a *healthy* joint. Friction in an *inflamed* joint may be followed by the most disastrous consequences. It is the fear of producing friction by the employment of this method that has hitherto prevented its use by British and Continental surgeons. The splints used in this method, especially that of Sayre, are frequently used in this country for a late stage of the disease, and when the patient is able to get about. Davis's splint consists of a metal rod, extending from the hip to the ankle along the outer side of the leg. It is interrupted about midway by a screw for regulating the length of the splint. To the lower end is attached the extending apparatus, and to the upper end the perineal band. The perineal band consists of an elastic layer and an unelastic

layer, so that tension is constantly ensured. Sayre's splint is essentially the same as Davis's, but instead of extending to the ankle, stops short at the knee. In the use of this splint, the lower portion of the thigh encircled by two curved pieces of iron is made the point of extension, and the perineum under which is passed a perineal band is made the point of counter-extension, tension is kept up by a rack work, moved by a key.—*Medical Press and Circular, March* 7, 1883, *p.* 200.

37.—ON DISLOCATION OF THE HIP-JOINT.

By Dr. Reuben A. Vance (in *Cincinnati Lancet*).

The questions involved in dislocations of the hip are neither few nor unimportant. I doubt if there be any topic in surgery of equal gravity in which there is such a diversity of opinion or contradiction in teaching. It is needless to inquire the cause. The complexity of the parts in which the lesion occurs, the different relations assumed by the bones of the pelvis and thigh in sitting, standing, and walking, together with the widely unlike circumstances under which dislocations of the hip occur, the readiness with which the ordinary land-marks can be obliterated by improper movements of the injured extremity, and the unfrequency with which *post mortem* examinations are made in individuals who have suffered this injury, all go to show why this state of facts should exist. This particular lesion has unfortunately been a favourite field for a class of theorists who adduce opinions without facts upon which to base them, and advance inferences which more frequently come from their imaginations than from their observations. In order to understand dislocations of the hip, the anatomy of the part must be studied by actual dissection, recorded cases must be analysed, the lesion itself investigated, and morbid specimens examined. In a word, it is from clinical and pathological investigations that the rules for the interpretation of recorded cases are to be derived; and, by converging the rays of fact emerging from each of these sources, surgeons will ultimately be enabled to illuminate this topic with the light of truth.

From anatomical, clinical, and pathological facts, I have deduced certain conclusions which are now offered for consideration.

The acetabulum is not equally strong at all points, but its osseous wall and cartilaginous and fibrous rim are so adapted to each other, that, at those points where the greater impacts are received and the most powerful pressures exercised, you find the excavation deepest and the osseous and cartilaginous embankment highest. The cotyloid notch is the weakest part of the acetabulum. This is directly downwards and slightly

backward, when the pelvis is in the position assumed when the body is erect, and is closed by the transverse ligament. As we pass from the osseous and cartilaginous to the ligamentous structure of the hip, we find precisely the same law in operation. Where the greatest tension is habitually exercised, there the ligamentous structures are best developed. The capsular ligament illustrates this principle admirably. In this structure the fibres pass from the brim of the acetabulum to the neck of the femur, a few strands coming from the anterior inferior spinous process of the ilium, and a few from the anterior aspect of the ilio-pectineal eminence. An unbroken continuity of fibres can be traced from the pubic border of the cotyloid notch upwards, backwards, and downwards, to the ischiatic border of the cotyloid notch. These fibres are thickest on the anterior aspect of the neck of the femur; those above and behind come next; while the weakest part of the capsule is that portion connecting the inferior aspect of the femoral neck with the transverse ligament. Before any conclusions are drawn from these facts, another circumstance should be taken into consideration: are there any structures in the neighbourhood that tend to prevent the direction of force against the weak point? Although the part of the capsule continuous with the transverse ligament closing the cotyloid notch is the weakest portion of that structure, yet the situation and action of the powerful adductor muscles so protect it, that luxation at this point is one of the rarer forms of dislocation of the hip. Weak as is this portion of the capsule, a luxation here can only occur during abduction; and the powerful adductor muscles, preventing that movement, keep the head of the femur from impinging on the under portion of the capsule. When adduction does occur, the head may penetrate its coverings just without the transverse ligament. The anatomy of the joint and surrounding parts demonstrates this fact: if the head of the femur be brought into contact with the capsule at any point, and sufficient force applied, a dislocation may result.

An examination of specimens, and a careful study of clinical phenomena, convince me that not only may you have a dislocation at any point, as anatomy teaches, but that in point of fact you do have the head of the femur forcing its way through the capsule at the point it chances to occupy when sufficient force is applied. The force that sends the head of the femur through the capsule, however, in the vast majority of cases, simply carries the articular extremity out of the socket, and does not tear away that part of the capsule opposite the opening. If the circle of the brim be divided into 360 degrees, it can truly be said that dislocations occur at every degree. The symptomatic phenomena characteristic of dislocations—

the flexion, inversion or eversion, shortening or lengthening, abduction or adduction, &c.—are mainly due to the portion of the capsular ligament that has not been ruptured by the force that dislocated the limb. The capsular ligament gives way at the point where the head of the femur impinges upon it at the critical moment; in the majority of cases there is a simple slit in the capsule, or at most a straight button-hole slit conjoined with a few fibres at the acetabulum, which give the capsular opening a T shape. As this opening will but rarely occur in exactly the same place in any two given cases, the symptoms developed by the traction exercised on the limb by the untorn portion of the capsule will seldom be alike in any two cases.

In order to comprehend the measures necessary for reducing a dislocation, it must be borne in mind that certain movements of the lower extremity are associated. Thus, when abduction occurs, this movement is naturally accompanied by eversion; adduction by inversion, &c. A man falling from a height to the earth flexes his legs upon his thighs, his thighs upon his body, and adducts his knees; that posture is instinctively assumed, in which the greatest amount of force can be received by the feet with the least damage to the organism. Should the force be greater than can be disposed of without harm, a dislocation may be produced. If the thigh be extended and strongly adducted, the head of the femur passes directly upward through the top of the capsule. It may now assume one of these positions; it may remain with the head hanging over the acetabulum, the head may advance, and the great trochanter recede; or the head may recede and the great trochanter advance. The dislocating force perforates the capsule and carries the head upon the brim of the acetabulum; the neck is surrounded by the borders of the rent in the capsule, and held firmly by the untorn portion of that structure; the subsequent movements are accidental, and may or may not occur. If the thigh be flexed and adducted at the moment that force is applied, the position of the head of the bone will vary with the amount of flexion and inward rotation. As before, so here, the neck will be held by the untorn portion of the capsular ligament, and the deformity that results will mainly be due to the tension it exercises. Should the force be applied while the limb is adducted, dislocation downwards will occur. Should the thigh be over-extended, adducted, and rotated outwards, force directly or indirectly applied will carry the head of the femur forward upon the pubes. Should it be forcibly flexed upon the body, adducted and rotated inward, it will emerge directly opposite the tuberosity of the ischium, and may rest upon the prominence. Through injudicious manipulation of the dislocated extremity, the head of the femur, when dislocated directly downwards, may be

carried towards the pubes or the ischium; and in certain cases, when force is used, it may even recede behind the ischium and pass without the internal obturator tendon. Between these typical and well-defined dislocations, there are many intermediate forms insensibly shading off into each other.

The principles which should govern the surgeon in his efforts at reduction can be thus formulated.

1. Place the limb in the position it occupied the moment it forced its way through the capsule, thus carrying the head of the femur opposite the opening through which it emerged.

2. Manipulate the limb in such a manner as to relax the untorn portion of the capsular ligament.

3. Draw or push, elevate or depress, the head of the femur in such a manner as to carry it over the brim of the acetabulum, exercising this force by proper movements of the extremity, directed by the grasp the surgeon has on the leg, at the same time so moving the limb as to keep constantly relaxed the untorn portion of the capsular ligament.—*London Medical Record, Nvv.* 15, *p.* 447.

38.—ON THE TREATMENT OF CERTAIN FRACTURES OF THE LOWER END OF THE FEMUR.

By Frederick Treves, F.R.C.S., Assistant-Surgeon to, and Senior Demonstrator of Anatomy at the London Hospital.

There are certain fractures, through the lower end of the shaft of the femur, that are apt to be associated with a peculiar deformity, and to require, in consequence, a special treatment. These fractures are, generally, either just above the condyles, or are directed through the shaft within a few inches of that spot. The fracture may be either transverse or oblique. It is more usually of the latter variety; the obliquity is often extreme, and its general inclination is from behind downwards and forwards. The more transverse fractures are probably due to direct violence; the more oblique, to a force indirectly applied. The position of the upper fragment is practically unaffected, while the upper end of the lower fragment is drawn forcibly backwards into the ham by the action of the gastrocnemii muscles. The lower fragment is, of course, always drawn upwards, and from the twofold displacement, it follows that it may cross the axes of the upper fragment at a considerable angle, while it will lie behind that portion of the bone. These fractures are very often associated with a secondary vertical fracture, that extends into the knee-joint, between the two condyles, and that thus produces what is known as the T-shaped fracture. It is not implied that all fractures, through the femur above the condyles, are associated with this dis-

placement; probably the majority are not. There are, indeed, many circumstances that would oppose such a displacement. There is, for instance, no reason why the obliquity of the fracture should not be such that this particular deformity would—after shortenings had occurred—be impossible. Many muscular and tendinous fibres are attached along the bone across the fracture line, and the deformity mentioned would not be possible, without a considerable laceration of these fibres. If the fracture takes place when the knee is bent, the condition of the gastrocnemii would be such that it would hardly be capable of forcibly dragging the lower fragment backwards at the time of injury. This is of consequence, since it would appear that, not a few of the fractures about this part, that are due to indirect violence, are caused by falls upon the bent knees. It is common, in museum specimens, to find the two fragments overlapping (the upper in front of the lower), and the upper end of the lower piece of bone directed a little backwards, so that the whole fragment is slightly oblique to the long axis of the upper portion of the femur. This trifling deviation may be due solely to the overlapping of the bones, the lower fragment being unavoidably directed backwards, as it passes up behind the upper fragment. The cases, however, to which reference is made in this paper, are those in which the lower fragment is drawn very conspicuously into the popliteal space, and where this displacement is, without much question, due to the gastrocnemii muscles.

It is obvious that these cases cannot be treated in the ordinary way. Extension in the long axis of the limb merely increases the deformity by dragging upon the gastrocnemius, and so upon the lower fragment. Thus, if the long splint were to be applied in the ordinary way, it would be possible for the limb to be put up apparently straight, while the knee-joint was all the time considerably flexed.

In one or two preserved specimens where the bones have united at a conspicuous angle, it is not improbable that this treatment had been carried out. It would be useless to attempt to act upon the little lower fragments by pads or pressure, and therefore the long splint being abandoned, the only available treatment is by some form of the double inclined plane. By the double inclined plane the knee is bent, and the large upper fragment is brought to the lower one; the parts can be well fixed to the splints, and the weight of the trunk can be made use of as an extending force. In applying this treatment, the actual double inclined plane is seldom made use of. It is cumbrous, difficult to maintain in a rigid posture, calculated to produce bedsores, and extremely uncomfortable. The principle, however, of the double inclined plane can be

applied in many less objectionable forms: as, for example, by a MacIntyre's splint bent considerably at the knee and slung from a cradle, by Nathan Smith's anterior wire splint, by Hodgen's suspension splint, and the like. In all these appliances the knee is bent, the parts below the fracture are practically fixed, and the weight of the body—the limb being suspended—acts as the extending force. Even to these ingenious forms of apparatus, a good deal of exception may be taken. The patient's body and the joint above the fracture are not fixed, and a somewhat undesirable degree of movement in the upper fragment is possible. The comparative freedom allowed by suspension increases this possibility. Moreover it is a question, when dealing with a fracture close about the joint, whether it be well to fix that joint in a flexed position, unless it be intended that it should always retain that posture. It must be remembered that some of these fractures are so near the joint, as to render unavoidable some damage to its soft parts, while many others actually enter the articulation. The prospect of ankylosis after the latter form of injury is not a slight one, and is well illustrated in those fractures of the humerus that involve the elbow joint.

Although, therefore, the treatment by the principle of the inclined plane may serve to remedy the immediate deformity, it is not without somewhat grave disadvantages when applied to these particular lesions. The treatment, on the other hand, that appears to most commend itself is that alluded to by Mr. Bryant (Practice of Surgery) in the following terms: "In fractures of the lower third above the condyles where the gastrocnemii muscles tend to draw the lower fragments backwards into the popliteal space, some surgeons prefer the use of the inclined plane, and where the bones cannot be otherwise kept in apposition, it is, probably, a sound practice. But what I believe will turn out to be a better one, is the division of the tendo Achillis, and the use of the long splint.....,I have taught this for the last twelve years, but have had only one opportunity of testing its value." By the method here advocated, any deformity produced by the gastrocnemii muscles can be entirely remedied, and the fracture treated with all those advantages that pertain to the use of the long splint. The treatment, indeed, recommends itself as a good surgical procedure on many grounds, and enables the surgeon to carry out those essential principles that, since the days of Pott, have been recognised as important in the treatment of fractures. Inasmuch as no details have, so far as I am aware, been given of this mode of dealing with the fractures described, I venture to record three cases in which I applied this treatment, and which appear to substantiate the advantages ascribed to it. The patients were all males; the

fractures differed in each instance both as to their nature and mode of causation; and in two of the cases the local result was rendered more evident by *post mortem* examinations.

Case 1.—R. P., a drayman, aged 33, was admitted into the London Hospital under my care on November 13th, 1879. He had fallen from his dray, and, while lying upon the ground, the wheel of the dray, which was heavily laden, had passed over his right thigh. The femur was fractured about two inches above the condyles, the fracture was slightly oblique from behind downwards, forwards, and outwards. There was some effusion into the knee-joint; the patella was a little displaced to the right; and the tibia, with the lower fragments, appeared to be so much displaced backwards as to give rise to an appearance not unlike that seen in dislocation. The lower end of the upper fragment projected forwards, and could be readily felt under the skin; the lower fragment was forcibly dragged backwards into the popliteal space. The vessels were uninjured. On applying extension, the deformity was much increased, although the shortening that existed could be almost entirely overcome. The limb was at first put up on a double inclined plane. This proceeding brought the fragments at once into good apposition, and removed the outward evidences of the deformity. The man however, was very stout, his weight being about sixteen stone; and it was soon found that, even with the greatest care, the parts would not be retained in position. He complained, moreover, a good deal of the pain occasioned by the apparatus. The limb was then placed in a Macintyre's splint, so that the knee should be considerably flexed; and the splint was slung from a cradle made for the purpose. The unwieldy size of the limb and the frequent movements of the patient (that the apparatus did not restrain) soon brought about a disturbance of the fragments; while the treatment, in addition, caused much inconvenience. On November 16th, I divided the tendo Achillis, and applied a long side-splint with a forty-pound weight attached; the lower end of the bed being raised to resist this extending force. The fragments were almost immediately brought into excellent position, and the whole extremity from the pelvis to the foot was firmly secured. The integument over the seat of fracture was so severely contused on admission, that I feared the skin would slough, and so make the fracture a compound one. I therefore dressed the part antiseptically as a precaution. A slough did form; but it remained for weeks dry and hard, and firmly attached. Not a drop of pus was produced. By January 10th, union appeared to be firm, and the shortening was one inch. The antiseptic dressings were discontinued; and in a short while afterwards the scab-like slough came away, leaving a granulating wound. The patient was

kept in until this little surface healed. He left the Hospital on March 1st, with the fracture firmly united, and able to walk without assistance of any kind. The knee-joint, however, was perfectly stiff. In this case, the great size of the limb seriously interfered with the suspension treatment; and, since the knee-joint became ankylosed, it is as well that it was allowed to become rigid in the straight position. Passive movement of the joint was attempted as soon as it was deemed safe, but with no result.

Case 2.—J. E., aged 46, admitted under my care May 26, 1880. The patient was said to have fallen forty feet, alighting upon his feet. There was a compound fracture of the right thigh at its lower part. The skin wound easily admitted the finger, and was situated about two inches above the patella; it had been caused by the penetration of the sharp lower end of the upper fragment. The fracture was very oblique, from behind, downwards and forwards, and was situate about two inches and a quarter from the condyles. There was also a vertical fracture extending between the two condyles into the knee-joint. The upper ends of the two lower fragments were pointed, and were drawn forcibly into the ham. There were many loose fragments of bone embedded in the adjacent muscles, and in the tissues of the popliteal spaces. The great vessels were uninjured. The sharp lower end of the upper fragment had been driven through the quadriceps, so that the whole thickness of that muscle intervened between it and the lower fragments. This mass of muscle was detached from its contact with the upper piece of bone with the greatest difficulty, and had it not been possible to manipulate the parts through the wound, this detachment would have been quite out of the question. From many various manœuvres with the limb, I am convinced that, had the fracture been a simple one, no manipulation would have dislodged the piece of muscle between the fragments. With the finger in the wound, the exact position of the broken bones could be well made out and easily studied. Before chloroform was given, I was surprised to find to what an extent the lower fragments had been dragged back into the ham. When the knee was fully bent, the two lower fragments could be readily moved, but when it was extended they became quite fixed. The two lower fragments were separated by a finger's breadth alone, but were in contact below. Keeping my finger upon the lower fragments, I tried the effects of various positions of the limb upon their movements. When the limb was extended in the straight line, the fragments were drawn forcibly into the ham, and the more the extension the more the displacement. Flexion over an inclined plane enabled the three fragments to come well together, but their mutual relations were considerably dis-

turbed by comparatively slight movement of the body. The tendo Achillis was then divided, and the long splint applied, with an extra dress weight of thirty pounds. The parts fell into excellent position, and could be firmly and evenly maintained there. The wound was dressed antiseptically. The patient was greatly shaken by the accident, and had received, in addition to the above injury, a very severe laceration of the arm, and a compound fracture of the left leg into the ankle-joint. He never rallied from the shock, but, in a few days, became delirious, and died eight days after his admission. At the *post mortem* examination the observations above made were verified at leisure.

Case 3.—R. B., basket-maker, aged 51, admitted January 4th, 1881. While under the influence of drink, the patient had slipped over a stone, and had fallen upon his hands and knees, striking the left knee against the kerbstone. There was a very oblique fracture of the lower third of the shaft, with a second fracture into the knee-joint that separated the two condyles. Both the lower fragments were found, on admission, to be drawn forcibly into the ham; and this deformity was exaggerated greatly by extension. The displacement had probably been effected by the patient's drunken attempt to rise. The limb was adjusted as above described. Shortly after admission, the patient had an attack of delirium tremens. During the attack, the value of the large splint, in allowing of the body being kept fixed was well demonstrated. Had any suspension splint been applied, it would have probably proved useless, as the patient was extremely restless. The section of the tendo Achillis at least prevented the gastrocnemius from taking any part in promoting displacement. The patient never recovered from the delirium, but gradually sank into a state of dementia, lost all control over his sphincters, became comatose, and died exactly thirty days after his admission. The *post mortem* examination revealed a somewhat remarkable fracture. It was oblique from behind downwards and forwards, and, commencing six inches above the condyles, ran down to the trochlear surface in front. In some part of the course, it was nearly vertical. The two corner fragments were made up of the two condyles, with a sharp spicule of the shaft attached to each. The fragment of the shaft remaining with the inner condyle was six inches in length; that remaining with the outer condyle, four inches. There were but slight inflammatory changes in the synovial membrane. The fragments were already united by a good deal of sound callus, especially at their upper parts, and were in very good position. The specimen is now in the Museum of the Royal College of Surgeons.—*British Medical Journal, Feb.* 17, 1883, *p.* 306.

39.—ON BONE-SETTING (SO-CALLED).

By R. Dacre Fox, Surgeon to the Manchester Southern Hospital, to the Police Force, and the M. S. and L. Railway.

This is the first time in the history of the British Medical Association that so-called bone-setting has been seriously discussed, and I think it is matter for congratulation that we have at last set ourselves impartially to examine the practice, notwithstanding that it is almost exclusively employed by a class of persons who are without our pale. The literature of bone-setting is scanty. Dr. Wharton Hood's handbook, giving an account of the late Mr. Hutton's method of setting free stiff limbs, is, so far as I know, the only attempt at a systematic exposition. There are, in addition, some papers scattered through the various journals, by Mr. Adams, Mr. Howard Marsh, and others, stating their opinion as to what cases are most likely to be benefited by the forcible bending of joints, and giving instances of ill results that have followed such treatment in unsuitable cases. For nearly three years I was assistant to the late Mr. James Taylor, M.R.C.S., of Whitworth in Lancashire, the last direct descendant of a family that had practised bone-setting in that village for more than two hundred years, and I think I may perhaps be able to throw some light on the subject of this discussion.

Much misconception exists as to the practice of bone-setters; many of the methods of treatment popularly attributed to them have no other existence than in the imagination of ignorant patients, whose stories we as a profession are perhaps rather too ready to believe. It is certain that some families—notably the Taylors, Huttons, and Masons—have by their manipulative and mechanical skill justly acquired a great reputation. In what has their practice consisted? First, in the treatment of fractures, recent dislocations and sprains; secondly, in the cure of stiff joints, resetting of fractures, and correction of deformities. The general impression in the profession appears to be that the bone-setter's art consists of nothing more nor less than the forcible and reckless "cracking up" of stiff joints so as to make the lame man walk as if by a miracle. The practice at Whitworth was a large one, furnishing constant employment for at least two active men, and consisting chiefly of the cases I have mentioned. Speaking from memory I do not believe that fifty joints of all sorts were "cracked up" during the time I was there; but it was not an uncommon event to have to put up half a dozen fresh fractures and twice as many recent sprains in a single morning. In the north of England the origin of nearly all the men who are fairly good at bone-setting can be traced to the Whitworth

surgery, and while, so far as I know, the Taylors in their various settlements at Whitworth, Todmorden, Lockwood, and Oldfield Lane were the only qualified surgeons who practised bone-setting; amongst the hills and dales of Lancashire, Yorkshire, and the Lake district, there were many who did so without being qualified, some of whom, I must in all fairness say, put up fractures uncommonly well. But apart from the legitimate credit they have won by the skill displayed in their handicraft, they owe some of their success to the carelessness or indifference of the general body of practitioners, who are apt to overlook little injuries which often become very painful and troublesome. It sometimes seems that it is beneath the dignity of the ordinary practitioner to employ any active treatment whatever for a sprain. It is hardly fair then to gauge the work of bone-setters solely by their method of treating diseased joints (probably the most unsatisfactory class of cases in the whole realm of surgery), but we ought also to take into account the patience and skill they display in the treatment of injuries for which they are not unfrequently consulted by the patients of qualified practitioners. I have no desire to hold a brief for every idle fellow who calls himself a bone-setter, but I am anxious to give credit where credit is due, and to explain that the *art* of bone-setting is not what it is often thought to be, a mere mixture of charlatanism and good luck.

I purpose chiefly to consider that class of cases to which Mr. Adams and Mr. Marsh have more particularly referred. Dr. Hood has made out a somewhat extensive list of causes for stiff or weak joints, but I think he had in his mind rather what was *likely* to come to the bone-setter than what actually did come. From my own experience I should classify weak joints as follows:—

1. Those that have become stiff from enforced rest.
2. Those that have become stiff from chronic disease.
3. Joints stiff from injury to the bones entering into their formation.
4. Joints stiff and weak from sprains, including displacement of tendons and partial luxation.

Apart from the previous history of the case, and the evident existence of constitutional disease, there are some external appearances which help to distinguish cases and to afford indications for treatment, and of these the bone-setters have learned by experience to avail themselves.

1. In the first class I have mentioned, the stiffness of the structures about the joint impeding its movements is the result of purely mechanical causes, is in fact simply due to prolonged disuse. No cause for functional activity exists, and consequently the elasticity, the flexibility and power of

adaptation to movement in the parts about the joints not being required, they become stiff and rigid. No degenerative changes, however, take place, and they are capable of being recalled into activity unimpaired. In such a joint the bony points and the outlines of the tendons and ligaments about it seem unnaturally prominent, probably from absorption of the adipose and connective tissue; the rigid ligaments impart a sense of hardness, and if the limb be flexed to its utmost it shows considerable resiliency. Such joints may, I believe, be "cracked up" without fear of consequences, and this constitutes one of the successful operations of bone-setters. My own recollection carries me back to some apparently almost miraculous results. I am convinced that *suddenness* ought to be insisted upon in doing this; the advantages derived from it being, I believe, mainly due to the fact that it is less likely to set up any irritation in the joint than the "dragging" of gradual extension.

2. In the next class of cases, in which stiffness is due to degenerative changes, the external appearances are exactly reversed, the outlines of the joint are more or less gone. In these cases, no matter the character of the disease, manipulative interference is positively vicious; and while it is in them that ignorant bone-setters do so much mischief, the better informed, by the use of splints and well-applied pressure, are highly successful in their treatment. I am sorry to say very many cases of this kind come to bone-setters which have not been properly treated before, owing to their not having been recognised, especially hip-joint disease.

3. In the third class of cases, in which a fracture has taken place into the joint, causing stiffness, the condition is due to disturbed relationship of the bones from faulty setting, and is recognised by comparison with the bony landmarks of the sound limb. In these cases forcible treatment does good, though of course the result is in proportion to the amount of bone-displacement, but it should be supplemented by passive movements for some time. In joints stiff after diagonal fracture through the condyles of the humerus, so common in children, I have seen many most gratifying results; one in a boy about twelve years old, whose elbow had been stiff three years, is especially impressed upon my mind.

4. In the fourth class of cases, and those to which I would draw particular attention, I include lameness and weakness, the result of the various forms of injury, which we group together under the general term "a sprain." I affirm most unhesitatingly, from an experience of some hundreds of cases, that nothing has done more to lower the prestige of regular practitioners, and to play into the hands of unqualified bone-

setters, than the way in which so many practitioners tamper with a sprained joint. Sprains, of course, vary greatly in severity; they may be broadly divided into two kinds, of which one consists merely of a temporary over-distension of the parts around a joint, which rest and anodyne applications soon cure, while the other involves pathological results of a much more serious nature. A *severe* sprain is the sum of the injuries that the parts in and about a joint sustain, when, by their passive efforts, they exercise their maximum power of restraint to prevent luxation. Under such conditions I conceive the following changes to take place in the integrity of a joint. In the case of the synovial membrane, temporary hyperæmia accompanied by pain, and some slight effusion into the cavity of the joint. In the case of the tendons, overstretching and loosening of the lining membrane of their sheaths, more or less disturbance to the adjacent cellular tissue forming the bed of the tendon groove, and hyperæmia with exudation of plastic fluid, subsequently forming adventitious products. In the case of the non-elastic fibrous ligaments—firmly attached at either end to the adjacent periosteum—overstretching, mostly involving partial rupture, with swelling, softening, and disintegration of their structure. It is beyond the purpose of this communication to draw attention to the plan of treatment adopted by bone-setters under these circumstances; it is, however, described in a paper of mine, of which an abstract is given in *Retrospect*, vol. 82, p. 134. The stiffness of a sprained joint is *partial*. The surface is generally cold, and more or less œdematous, and each joint has one particular spot in which pressure causes acute pain; the bone-setters have learned by experience the situation of these spots, and this fact has done more than anything to strengthen the popular faith in their intuitive skill; they certainly form an important guide to treatment, since they indicate the seat of greatest injury to the ligaments and point out where their power of passive resistance has been most severely tested, and where adhesions are most likely to have formed. Dr. Hood, in his record of Mr. Hutton's practice, has enumerated some of these painful spots; the chief of them are as follows:—

1. Over the head of the femur in the centre of the groin, corresponding to the ilio-femoral band of the capsular ligament (which is most severely stretched when the thigh is over-extended, as when the trunk is flung violently backwards, the commonest cause of a sprained hip).

2. For the knee-joint, at the back of the lower edge of the internal condyle—in other words, at the posterior border of the internal lateral ligament, where it blends with Winslow's ligament, and where the semi-membranosus tendon is in intimate

relation with it. These parts suffer most because, as Mr. Morris says, "During extension they resist rotation outwards of the tibia upon a vertical axis," and a sprained knee is almost always caused by a twist outwards of the foot.

3. For the shoulder at the point corresponding to the bicipital groove, because in nine cases out of ten a man sprains his shoulder to prevent himself from falling, his hand grasps the nearest support, the body is violently abducted from the arm, the long head of the biceps is called upon to exert its utmost restraining power, the bicipital fascia is overstretched, and the tendon very often displaced.

Again, for the elbow the painful place is at the front of the tip of the internal condyle; the fan-shaped internal lateral ligament has its apex at that point, and it is most stretched in over-supination, with extreme extension of the forearm. On the front of the external malleolus, at the apex of the plantar arch, the tip of the fifth metatarsal bone, the styloid process of the ulna, the inside of the thumb, and the annular ligament in the front of the wrist, are respectively the most painful spots when those joints are severally sprained.

The manipulative part of the treatment of joints stiff from being sprained may be briefly said to consist in pressure over the part most injured, and momentary extension of the limb, followed by sudden forcible flexion. The method of doing it varies with each joint, and I can with confidence refer you to the descriptions given by Dr. Wharton Hood, as being faithful word-pictures, supplemented, too, by very accurate drawings.

The following are some of the lesser injuries the non-recognition of which has frequently come under my notice at Whitworth. In the upper limb: Fracture of the tip of the acromion; partial luxation of the acromio-clavicular and sterno-clavicular joints (often happening to men who carry weights on their shoulders); partial dislocation of the long head of the biceps, with over-extension of the bicipital fascia (common in men who throw weights or use a shovel, as maltsters or navvies). Dislocation of the head of the radius forward on the condyle, which is very common in children, and has a marked tendency to cause stiff elbows; fracture of the tip of the internal condyle; overlooked Colles' fracture; partial luxation of the head of the ulna (impeding supination of the hand, and having a tendency to gradually grow worse); severe sprain at the carpo-metacarpal joint of the thumb (very common in stonemasons, and caused by the "jar" of heavy chisels).

In the lower limb: Fracture of the fibula just above the malleolus and at its tip (these are fruitful sources of lameness, often overlooked, and, if of old standing, very troublesome to treat); partial rupture of the ligamentum patellæ at its inser-

tion into the tubercle of the tibia, which is much more common than is ordinarily supposed; neglected overstretching of the ligaments of the plantar arch, and tearing of the plantar ligament at its insertion into the os calcis; rupture of the penniform muscular attachments of the tendo Achillis, and muscular hernia in the calf.

I am sure so-called *trifling* injuries deserve more attention at our hands, since living at the high pressure men do nowadays, with every part of their bodies tested to its utmost capacity, the slightest impairment of the mechanism of a limb must be an incalculable source of personal annoyance, discomfort, or disability.—*Lancet, Nov.* 18, 1883, *p.* 843.

40.—A NEW OPERATION FOR SPINA BIFIDA.

By A. W. Mayo Robson, F.R.C.S., Assistant Surgeon to the Leeds General Infirmary, &c.

[The method of operating advocated by Mr. Robson is illustrated in the account of the following case.]

Mrs. S. consulted me concerning her child, a few days old, which was the subject of a spina bifida in the lumbar region, of the size of a tennis-ball. She told me that, during the early part of her pregnancy, she had been very much "out of sorts," and had eaten very little. This, I think, may possibly have had something to do with the deformity in the child—just as underfeeding a pregnant mother may produce hare-lip in the infant. The tumour had such thin walls that, in certain parts, it was perfectly transparent. There was a distinct impulse on coughing or crying; and the communication between the dilated sac and the spinal canal could be felt to be more than an inch in length, and over half an inch wide.

I advised an operation, but suggested that it should be done after a month or two, in order that the child might gain strength; but, on October 26th—*i.e.*, when it was six days old—the skin had become so red and thin over the upper part of the swelling, that I saw it would burst if left a day longer: hence I decided to operate at that early period. Mr. Wm. Hall kindly gave chloroform, and, when the infant was fully anæsthetised, I made a vertical incision on each side of the tumour, about half an inch from its base, through the skin, and then very carefully dissected the integuments from the meninges, until I reached the laminæ of the vertebræ; this required very careful dissection, as the membranes left were so thin as to be perfectly translucent; the fluid was now let out by puncturing with fine scissors, which were also used to cut away the redundant membranes. The cauda equina was fully exposed, lying on the floor of the spinal canal. I now had two folds on each

side, each fold being of a different width, the two inner meningeal folds three-fourths and half an inch respectively, and the two skin-flaps of the same width; but whilst the wider meningeal flap was on the right, the wider skin-flap was on the left. Thus, when sutures were applied, the lines of union were not opposite. Acting on the same principle as is carried out in uniting the peritoneum, I brought together the serous surfaces of the arachnoid by several sutures, so as to completely shut off the spinal canal.

Mr. Mayo had, in the meantime, been kindly dissecting (under the antiseptic spray) the periosteum from the femur and frontal bone of a rabbit, which he had just killed. This periosteum I now placed, with its osteogenic layer undermost, over the closed meninges, and carefully sutured it to the periosteum of the laminæ on each side, and to the bony margins above and below. After this the skin was sutured, a layer of protective applied, and a pad of salicylic wool placed over the wound. The whole operation, which occupied more than an hour, was performed under the eucalyptus air. Catgut ligatures were employed, and the instruments and sponges were well carbolised. On the second day, the nurse, in applying the napkins, displaced the dressing; but although the skin-wound slightly opened, there was no formation of pus, and no slough came away; in fact, through the small opening, I could see that granulations had sprung up from the superficial surface of the interposed periosteum. The child has thriven, and has not had a single bad symptom.

As yet I cannot feel any bony crackling, but the skin is level with the surface, and the case is practically cured; if bone form however, the covering will be all the firmer, and the spinal canal will be physiologically perfect.

I had mentioned my intention to Mr. Jessop, a short time before I performed the operation, and he kindly offered to let me have some periosteum from an amputated leg; but, unfortunately, the inflamed condition of the sac gave me no chance of choosing my time. The capability of grafting living tissues, and seeing them still live, has been proved in the case of cuticle on the surface of ulcers and skin in cases of ectropion; the special instance which suggested the grafting of periosteum to my mind, was the witnessing of the continued vitality of two flaps of skin which I detached from the forearm and transplanted to the nose, in a case of nasal deformity.

The points of interest to which I would draw attention are: 1. The performance of the operation with strict antiseptic precautions, the eucalyptus air being used instead of carbolic spray. 2. The principle of closing the meninges, by bringing together the serous surfaces, as in peritoneal surgery. 3. The

possibility of transplanting periosteum, and its continuing to live. 4. The entire absence of adverse symptoms. 5. The illness of the mother during early period of pregnancy, and its questionable bearing on the condition of the child.

The following description of the specimen, which is preserved in the Leeds Medical School Museum, has been furnished by the curator, Mr. F. H. Mayo. The sac, somewhat contracted from being in spirit, is about the size and shape of half a swan's egg, being $2\frac{1}{2}$ inches in length, from above downwards; $1\frac{3}{4}$ inches in breadth, from side to side; $1\frac{3}{4}$ inches in depth, from base to apex. The sac-wall consists of true skin and subcutaneous tissue, lined by serous membrane, and is about the thickness of ordinary skin, except at one point, about the size of a sixpence, situated just to the right of the apex, where it is so thin as to be quite transparent, appearing to consist only of the serous membrane and a very thin layer of epidermis, and where fresh minute blood-vessels could be distinctly seen ramifying over it.

Feb. 10th, 1883.—It is now four months since the operation, and two months since the child was shown to the Leeds and West Riding Medico-Chirurgical Society. It has thriven well, and is now strong and healthy. The skin over the lumbar region is quite flat with the rest of the back, and the site of the tumour is only marked by the line of incision. Apparently no new bone has formed, since the structures covering the spinal hiatus feel soft. They seem to be thicker than the skin and meninges employed to cover in the gap; therefore, it is just possible that the transplanted periosteum has survived, and now renders the covering more firm than it otherwise would be. Although one cannot help feeling disappointed that new bone has not formed, I shall hope to have better success when I can transplant periosteum from a recently amputated limb, and not from one of the lower animals. With or without the transplantation of a bone-forming membrane, this method seems to me to be worthy of further trial; as in the above recorded case, the cure is as good as can be desired.—*British Medical Journal*, *March* 24, 1883, *p.* 558.

41.—ON A CANNULATED NEEDLE FOR INTRODUCING WIRE-SUTURES IN SURGICAL OPERATIONS.

The annoyance caused by the wire suture catching in the tissues, on account of the doubling and twisting necessary to retain it in the eye of the ordinary needle, is often very great, and the excessive oozing caused by the procedure most injurious.

In order to overcome this difficulty, Dr. George McClellan of Philadelphia had a little instrument constructed for him by

Mr. Snowden, surgical cutler, No. 7, South 11th Street, which he has been constantly employing for several months in all operations where the approximation of the edges of the wounds required the introduction of wire sutures.

It consists of a long slender cannulated needle, having a slit at the point and another at the shoulder, where it joins the handle. There are two slits or openings in the needle through which the wire is passed. In introducing the cannulated needle, the wire is of course either retracted or withdrawn; but, as soon as the point issues at the desired place, the wire can be pushed forward, seized, and the needle withdrawn.

Although the idea of such a needle was entirely new to Dr. McClellan when he first had it constructed, he has since learned that it was suggested and used by several surgeons many years ago. Dr. W. L. Atlee used a needle on this principle in his operations for ovariotomy, and Dr. Paul B. Goddard had needles of various sizes and forms for the purpose of passing wire sutures; but neither of these surgeons appears to have encouraged their general use.

The ordinary forms of needle, including the 'screw-hole', and the 'gutter-eyed,' as well as the more recent suggestion of Dr. Morgan of Baltimore, all require considerable time in their manipulation, and in many instances must be grasped by a holder, that they may be properly introduced. In all of them the wire is apt to become caught in the tissues, or dislodged from the eye or stylet, unless great care be taken to prevent it. The advantages of such a cannulated needle will be apparent at a glance. The simple direct puncture of the needle will be found to occasion very little oozing; and, as the handle offers firm support in the hand of the operator, great accuracy in the introduction of the sutures and complete exactness in the apposition in the parts are attained, the needle being made to transfix both margins of the wound at the same time, and the wire, when passed through, taking its place.

The tearing of the knotted wire about the eye of a needle often causes a laceration instead of a puncture; and in some regions, as in the perinæum or vagina, this minute rent is very apt to tear further when the sutures are tightened. The wire should always be carefully straightened by running it over the edge of the thumb nail, to avoid any kinks which might interfere with its passage through the cannulated needle. With this precaution, it may then be introduced with much greater rapidity and precision than in any other way.

Dr. McClellan has also found that this little instrument is very useful in taking the place of a tenaculum or artery-forceps, either of which often loosens or tears away the ligature when it is being removed. He simply passes the needle through a bleeding

point, then forwards the wire and withdraws the needle, leaving the wire so that it can be doubled into a loop, traction upon which will enable an assistant to throw a ligature completely round the vessel or bleeding point, as readily as a tenaculum; with this advantage, that as soon as the vessel is secured the wire may be cut, thereby avoiding the danger of displacing the ligature. This form of needle may also be used for the purpose of exploring. Upon introducing the point into a part where blood, serum, or pus is suspected to have accumulated, the fluid, if any exist, will pass through the needle and appear at the slit near the shoulder.

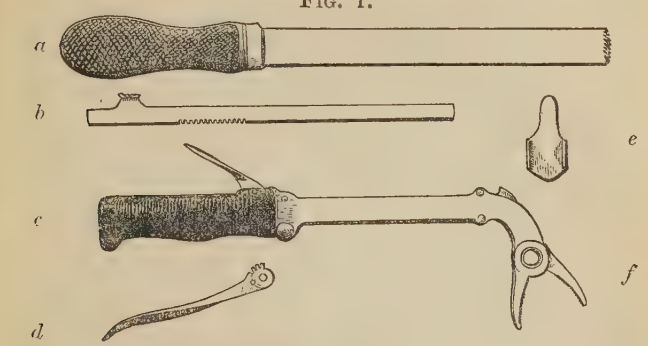
Objection may be raised to this form of needle on the score of the possibility of its conveying septic matter, but the same fault may be found with the hypodermic syringe, and it can be readily remedied by always retaining a piece of wire within the cannulated needle when out of use, and never forgetting to dip it in carbolised oil, both before and after it has been employed. The needle can readily be made straight or curved, as may be desired.—*London Medical Record*, *Nov.* 15, *p.* 480.

42.—A NEW OSTEOTOME, AND ELEVATORS.

The Osteotome figured in the diagrams below is the invention of Mr. B. Campbell Gowan, a student at Guy's Hospital, who, it is almost needless to say, has devoted considerable time to the pursuit of practical mechanics before entering the medical profession. The instrument consists of two main parts. The first is a forceps (Fig. 1, *c*) with jaws working in a plane at right angles to the long powerful handle. With this the bone to be sawn is held. To render the hold on the bone secure, a thrust-rod (*b*) runs in the upper part of the handle, and its end presses upon and actuates the male or distal jaw of the forceps (*f*). On the upper edge of this rod is a thumb-piece, and on its lower edge it is toothed and is brought into gear, with teeth on the pinion of a lever (*d*), which is fitted on below the handle. When the jaws of the forceps grasp the bone, the rod is thrust forwards with the thumb, and then by raising the lever (*d*) in the palm of the hand, the thrust-rod is fixed and pressed still further home, and the jaws of the forceps thus made to grasp the bone immovably. The second part of the instrument consists of a long saw-blade and handle (*a*). The teeth are cut on the end of this blade, are double, and have a slightly forward direction. This is fixed on to the forceps by a pivot-slide (*e*), which has a pin on its back (not shown in the diagram), which passes through a bush in the centre of the jaws, and keys the saw on to the forceps. On the front of the pivot-slide are lip-flanges, and the sides of the saw-blade fit accurately into them. With the forceps fixed on to the bone in the desired position and plane, the saw-blade is slipped down into this

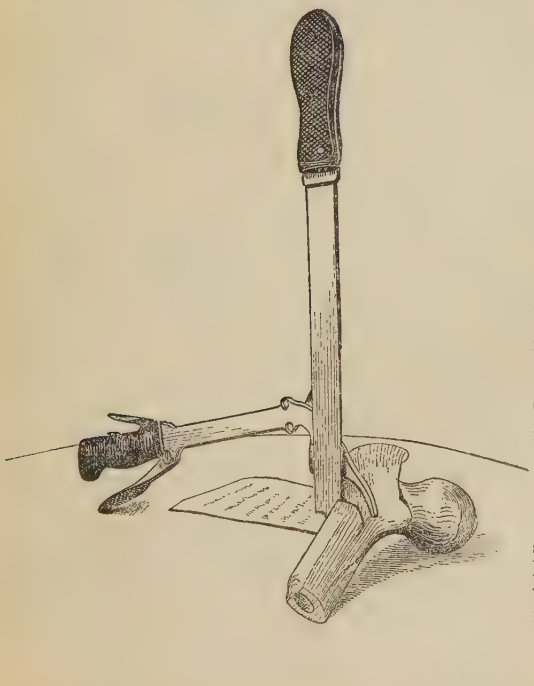
pivot-slide, and on until its teeth rest upon the bone. The saw is then at right angles to the handle of the forceps, and parallel

FIG. 1.



with the blades, as shown in Fig. 2. The surgeon grasps

FIG. 2.



the forceps tightly in one hand, and works the handle of the saw to and fro in the only plane in which movement is free —viz. parallel to the sides of the jaws,— and the bone is rapidly and accurately sawn across. The instrument has been used at Guy's Hospital, and found to answer well. Its advantages are the firm hold on

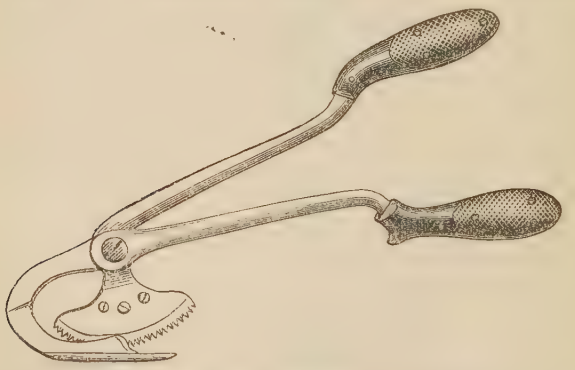
the bone, and the ease and rapidity with which a true section of a bone can be made without injury to the soft parts.

Mr. Gowan has also devised Elevators for operations upon bones. They are so shaped as to be easily passed beneath a bone, one on each side, and their curved ends fit into each other. When in this position, by depressing the handles the bone lying on the other ends is at once pressed up into the wound.

The above instruments, which promise to be of considerable value, are made by Mr. Hawksley, of Oxford-street.—*Lancet, Nov.* 11, 1882, *p.* 810.

43.—REMOVAL OF PLASTER-OF-PARIS SPLINTS.

Mr. Gowan, of Guy's Hospital, has designed a very useful instrument, which is capable of saving much labour in that often troublesome and dirty job of removing plaster-of-Paris splints. In principle it is not unlike some others we have seen, but is more powerful and workmanlike in construction. It consists of two hinged blades, one of which, the female, is prolonged into a blunt-ended beak, which is slipped under the



edge of the splint; its upper surface is grooved, and in this groove works the extremity of the male blade, which is a circular saw. The handles of the blades are long, so as to afford considerable leverage. In use the beak is passed up under the splint, which is then quickly sawn through by a to-and-fro movement of the male handle. Those who use plaster-of-Paris splints and apparatus will find this a very useful instrument. It is made by Mr. Hawksley.—*Lancet, Jan.* 20, 1883.

ORGANS OF CIRCULATION.

44.—ON THE SURGICAL USES OF KANGAROO TENDONS.

By T. M. GIRDLESTONE, F.R.C.S., Lecturer on Surgery in the University of Melbourne, Victoria.

For tying large vessels in their continuity the long even tendon from the tail of the kangaroo possesses decided advantages. It is as strong as the ordinary silk ligature, its knot is equally secure, and it causes no ulceration in the coats of the vessel. It has all the valuable qualities of the catgut ligature without any of the defects.

A fine tendon makes also an excellent suture, it produces no more irritation than a gut suture, and it resists the softening effects of purulent discharges for a much longer period.

In 1877 I brought the peculiar properties of these tendons under the notice of the profession in Melbourne at a meeting of the Medical Society of Victoria, and since that time they have been in frequent use there, in place of silk or gut, as ligatures and sutures. At the Melbourne Hospital they have been used on different occasions for the deligation of main arterial trunks, as the common carotid, the femoral, &c., and they answered every requirement. The inner coats of a vessel can be divided or not, as the operator pleases, and the ends of the ligature may be cut short off. A reef-knot which has been fairly tied in a tendon will neither slip, nor will it become loose, under any circumstances. In a wound where there is little or no suppuration the tendon appears to coalesce with the living tissues. Even in suppurating wounds, where they were bathed in septic discharges, I have found the tendon sutures partly softened but still holding well together, at the end of eight days. In all the cases here referred to the tendons were previously prepared by immersion for some weeks in carbolic oil, after the manner recommended by Prof. Lister for carbolising catgut. Some of our operations were performed under the antiseptic method, but others were not. *Entire* tendons only were used, those which were split being always rejected. In the hospital carbolised tendons have been also employed for making the deep stitches in ovariotomy; also for bringing together the cut edges of the vagina in the operation for the cure of vesico-vaginal fistula; in both cases they held the parts in apposition till perfect union took place. In plastic operations, in operations for the radical cure of hernia, and for the cure of varicocele, I have left carbolised tendons in the tissues, which have readily healed over them. A tendon ligature which I had applied to the carotid artery of a dog was found, ten days afterwards, when the animal was killed, perfectly strong, and the knot as secure as

when it was tied, while the tendon itself was incorporated with newly formed material, and adhered firmly to the outer coat of the vessel. In this case, as one might suppose, antiseptics were not employed, and the dog tore out the sutures which held the skin edges of the wound together; therefore at the end of ten days only the deeper parts and the opening which had been made in the sheath of the vessels were healed; the rest was healing by granulation, but the ligature was not softened.

A medium sized tendon (*e.g.* about as thick as a catgut ligature which would be selected for the deligation of a large artery) is so strong that it is difficult to break it with your hands; both its strength and calibre are uniform throughout its entire length, which ranges from twelve to eighteen inches. It is owing to the peculiar flattened form, and perhaps also to the fibrous surface of the tendon from the kangaroo's tail, that a reef-knot tied in it retains its grip when left in the interior of a wound.

Like catgut, tendons can be hardened, if desired, before they are employed as sutures in certain operations, by previous immersion, for about seven hours, in a half per cent. solution of chromic acid. This preparation may sometimes be necessary, but it was not adopted in any of the cases just referred to. When hardened by chromic acid they make excellent drains for wounds, and I have often so employed them.

Their preparation for surgical purposes is most simple, but it is necessary to mention that they should not be removed from the tail *en masse*, they must be taken out one at a time, without force, or they will sometimes split longitudinally, which greatly deteriorates their strength. *A split tendon cannot be relied on, and should never be used.* In their normal state they are of various sizes; some are thick, others very fine, in fact every diameter which can be required is obtainable; there is, therefore, no occasion for splitting. Neither is it wise to twist two or more tendons together, as it destroys their flattened forms.

On removal from the recently killed animal they are cleaned first in water, afterwards in a carbolic acid solution, and then dried. They can be preserved dry, or in five per cent. carbolic oil, whichever is most convenient. It will be necessary to steep them again in a watery solution of carbolic acid before they are used.

Some tendons which I sent to London a few years ago were exhibited by the late Mr. Callender at the Clinical Society, and Mr. T. Smith, of St. Bartholomew's Hospital, speaks well of them. But as it has been said that they are not at present procurable in England, I wish to add that if they are required the difficulty of obtaining supplies from Australia is almost *nil.*—*Medico-Chirurgical Transactions, vol.* 65, 1882, *p.* 71.

45.—ON THE TREATMENT OF CLEFT PALATE.

By Christopher Heath, F.R.C.S., Holme Professor of Clinical Surgery in University College, London.

The operation of staphyloraphy for closure of a cleft palate was, up to the last fifteen years, almost invariably postponed until the period of puberty or later, so that the surgeon might have the patient's self-control to aid him in his troublesome and tedious operation. A few cases had, no doubt, been operated upon in childhood, with indifferent success, by Mason Warren, and others; but to Mr. Thomas Smith, of St. Bartholomew's and the Children's Hospitals, is mainly due the credit of showing that it is possible to operate in infancy under chloroform, not only without risk, but with very great success. In his paper in the Med. Chir. Trans. (1868), Mr. Smith described a gag by which the mouth of a patient can be kept open efficiently, and which, in some form or other, is essential for the due performance of the operation, now performed by him as follows. The patient is placed on a table of convenient height, facing the window and, if possible, a northern light. The head is supported by an air-cushion, and is firmly held by an assistant standing behind, while the arms and legs are strapped down to prevent struggling. The operator stands on the right of the patient, and the chloroformist on his left. The patient being thoroughly narcotised, the gag is introduced and the mouth screwed open, the rings of the gag being held by the thumbs of the assistant supporting the patient's head. The edges of the cleft are then pared with a slender double-edged knife, which is thrust through the margin of one side of the soft palate held tense with forceps, and made to cut up and then down to the end of the uvula. The margin thus separated is caught with the forceps, and the section completed up to the angle of the cleft, if possible, at one sweep, or if not, by a re-application of the knife. The same process is repeated on the other side; and, in favourable cases, it is both possible and satisfactory to remove the parings of both sides of the palate in one piece.

The closure of the soft palate is then proceeded with, fine silver-wire being used, with horsehair or silk for the uvula. Mr. Smith employs a sharply curved tubular needle for the wire, which is carried on a wheel in the handle of the instrument, and can be projected when the point has traversed both sides of the palate. A twister is employed to twist the wire up, but the last two turns are more conveniently given at the conclusion of the operation with a pair of torsion-forceps. The horsehair is softened in warm water, and is introduced with a small

curved or rectangular needle set in a handle, being passed through both sides of the palate, and caught with a "catcher" or forceps. The horsehair is simply tied with three knots, so as not to slip.

Should there be a fissure of the hard palate, the operator proceeds to detach the muco-periosteum by a modification of Langenbeck's urano-plastic method. Making a puncture near the alveolus, he introduces a palate-rasparatory of small size, and brings the point out in the fissure. Then, meeting this with a strong aneurysm-needle, he replaces the raspatory with the needle, withdrawing the former altogether, and working with the needle from the fissure towards the alveolus. In this way, aided, if necessary, by the leverage of strong curved scissors, the muco-periosteum of the hard palate is sufficiently detached; the hemorrhage, if troublesome, being controlled from time to time by a small sponge pressed against the palate. With the curved scissors, the soft palate is then detached from the bone of the hard, one blade being passed beneath the muco-periosteum, and the other above the soft palate, so as to divide the tissues transversely close to the horizontal plate of the palate-bone.

The muco-periosteal flaps of the hard palate are now closed by a sufficient number of fine wire sutures; and, lastly, an incision, as recommended by Dieffenbach, is made on each side of the soft palate, so as to thoroughly loosen it, and take all tension off the stitches, which are then finally adjusted with torsion-forceps.

In this operation, no formal division of the levator palati muscles, as recommended by Sir W. Fergusson, is undertaken; but it is probable that the lateral incisions in the soft palate divide, to a great extent, the insertions of the levators; while the detachment of the soft from the osseous hard palate must necessarily divide the insertion of the tensors of the palate.

Many operators, myself among them, have found that they have obtained good union of the soft palate without even lateral incisions; but, when the tension is great, these should not be omitted; and in cases of great muscular irritability of the palate, the systematic division of the levators may be undertaken with advantage. Whether the division is performed, as recommended by Sir W. Fergusson, with a lancet-shaped blade set at right angles to the shaft of the knife, and introduced above the soft palate, or with a straight knife thrust through the palate, as proposed by Mr. Pollock, the division should be performed after the introduction of the stitches, which both serve to render the muscles tense, and also are more readily introduced before the hemorrhage, sometimes severe, caused by the myotomy, obscures the view.

The material for, and the method of introducing, the stitches, vary in the hands of different surgeons. Sir W. Fergusson preferred purse-silk, and employed the method devised by Mr. Avery for introducing it, as follows. An ordinary palate-needle carrying the silk was passed from before backwards through the margin of the soft palate, and the loop caught with forceps and drawn out of the fissure. This being repeated on the opposite side, one silk was looped through the other and drawn by this means across the fissure and through both sides of the palate. In tying the silk, it is convenient to use a sliding knot—*i.e.*, one end is simply knotted firmly upon the other, when, by pulling the silk, the knot is slid up to the palate, and will keep its place while a second knot is made over it.

In the after-treatment of a case of operation for cleft palate, it is essential that plenty of liquid nourishment should be given in the first day or two, and be followed up by semi-solid food, so that the patient's strength may be well supported, and the process of healing accelerated. The idea that any effort at swallowing, even of the saliva, must necessarily tear open the wound, was shown, by Sir William Fergusson, to be fallacious; and the practice of starving the patient, first decried by Sir Philip Crampton, is now universally abandoned. Talking should be forbidden for the first few days; and it is undesirable that the patient should be exposed to any chance of taking cold by exposure to draught or east wind.

The time for removing the stitches after an operation for cleft palate has been greatly modified of late years, and it has come to be thought by the most experienced operators that the longer they are left the better. In most cases, silk or horsehair sutures should be removed in from ten days to a fortnight; but fine wire sutures may be left for weeks, or even months, so long as they do not scratch the tongue.

In order to improve the voice in cases of successful staphyloraphy in which the nasal tone persists, Mr. Francis Mason has proposed to divide the united soft palate on each side by a vertical incision, so that the soft palate may be loosened and made more flexible. The results are, however, disappointing, as the cicatrisation which necessarily ensues leaves the parts much *in statu quo antea*.

M. Passavant, with the same object in view, has proposed to make a transverse incision in the soft palate, which can then be drawn forward and reversed. A portion of the mucous membrane of the upper surface is then dissected off, and a corresponding portion of the mucous membrane of the pharynx, and the two raw surfaces are brought together with a few sutures. As M. Sédillot remarks, however, it is difficult to understand how the two surfaces can be brought into contact; and the

experience of all surgeons goes to show that permanent separation of the posterior nares and pharynx by cicatricial tissue is certain to produce nasal intonation.

In describing the operation usually performed by Mr. T. Smith, it will be noticed that the entire fissure in both hard and soft palates is closed by the same operation. This practice is not followed by some surgeons, who content themselves with closing the soft palate first, hoping thus to influence the approximation of the two halves of the hard palate, or, in cases too wide for closure, to employ an artificial palate. It seems to me, however, that it is very undesirable to close the soft palate when the hard palate cannot be closed, for in these severe cases the soft palate is both small and short, and the results, therefore, as regards the voice are most unsatisfactory, even after the gap in the hard palate has been filled artificially. Again, if the patient is to be subjected to the inconvenience of wearing an artificial palate at all, he may as well be fitted with one to fill up the whole cleft, and thus improve the voice, which will be impossible if the scanty soft palate has been united.

The late Dr. Mason Warren, of Boston, was one of the pioneers in closing fissures of the hard palate by dissecting down a flap of mucous membrane, and his success was such as to encourage other surgeons to imitate his practice. He worked from the margin of the cleft, using sharp knives set at a right angle to detach the periosteum, which was then united with sutures. Mr. Avery was the first to follow his example in England.

In 1862, Langenbeck published his experience of operating from the alveolus towards the margin of the cleft, and laid especial stress upon detaching the periosteum by means of blunt raspatories to form part of the thickness of the flap. Having performed this operation on several occasions with good success, I can speak well of it, notwithstanding the incision near the alveolus, which may, if carried too far back, give rise to troublesome hemorrhage. Two instances, in which nearly fatal hemorrhage occurred from the posterior palatine artery, have been recorded by Mr. Howard Marsh, in both of which the hemorrhage was successfully arrested by plugging the palatine canal with a wooden plug pushed through the palate.

In 1874, Sir William Fergusson brought forward what was then believed to be a new proposal, but was afterwards shown to be a revival of a suggestion of Dieffenbach's, viz., to detach the bony edge of the hard palate with a chisel, and to push the two portions of the hard palate towards the median line. In his early cases, Fergusson used silk sutures, but in his later ones he found he gained sufficient approximation by stuffing

the gap made on each side with lint. In this proceeding the damage done to the parts is considerable, and necrosis, with some exfoliation, is apt to follow. Mr. Mason has proposed to limit the action of the chisel by boring holes in the line of incision with a brad-awl before the chisel is applied; but even with this modification the operation is one of limited application, which has not found favour with many surgeons.

Although the surgeon will not undertake to supply artificial aid himself in cases of deformity of the mouth, it is essential that he should be able to advise his patient in the matter, and possibly direct the dentist or mechanician as to the method to be adopted. In Mr. James Salter's and Mr. Oakley Coles' works, there will be found complete historical accounts of the methods adopted by successive practitioners to obviate mechanically the deformities of the palate; but for surgical purposes it will be sufficient to say that, as regards congenital cleft-palate, it is only during the last sixty years that anything like success has been attained. In 1820, M. De la Barre seems first to have employed "elastic gum" in making artificial palates, and in 1828 Mr. Snell made a palate of gold with a movable velum, which was subsequently improved by Stearn (1845), Sercombe (1857), and Parkinson (1867). All these palates acted simply as obturators, *i.e.*, were placed below the palate, and overlapped the margins of the cleft, thus more or less completely shutting off the nose from the mouth. In 1864, Dr. Norman Kingsley, of New York, brought before the Odontological Society of Great Britain an artificial palate made entirely of vulcanised rubber, cast in moulds of metal taken from impressions in plaster-of-Paris. The velum of soft rubber had the great peculiarity that it fitted between the halves of the split palate and moved with them, at the same time filling up the gap between the palate and back of the pharynx. With this palate, adapted by Mr. Coles, I have seen very remarkable results; but the method has the drawback that, if applied to patients whose growth is not complete, frequent alterations are required, in addition to the fact that the soft rubber wears out and requires renewal.

A palate on a totally different principle, contrived originally by Dr. Wilhelm Suersen, of Berlin, and lately brought to my notice by Mr. Oakley Coles, appears in many cases to afford the most satisfactory means of treating cases of wide congenital cleft of the palate. It consists entirely of hard rubber, the posterior part being much thicker than the hard palate, to which it is attached by a narrow stem. The posterior part, which is triangular shape, is set at an angle so as to pass above the soft palate, and fill up the cavity of the pharynx, the edges of the soft palate coming in contact with it in deglutition and

phonation. Mr. Coles has found that the tone of the voice is improved by substituting gold in the hard palate, and having induced a highly intelligent medical student to try one of these palates after having worn one of Kingsley's, he informs me that he has no hesitation in giving the preference to the former, whilst I am able personally to testify to the improvement of his voice.

Whatever method of treating a congenital cleft palate may be adopted, the improvement in the voice must be gained to a great degree by education. A patient has in the first few years of life acquired vicious habits of speaking, and particularly a guttural and nasal tone, which requires great care to overcome. It is essential that he should be taught to speak with his lips, and to throw the voice forward; and, with proper teaching and diligent practice, he will in time speak as distinctly as the majority of healthy persons.—*British Medical Journal, Nov.* 4, 1882, *p.* 874.

46.—ON THE USE OF CHROMIC ACID IN CERTAIN AFFECTIONS OF THE TONGUE.

By Henry T. Butlin, F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital.

Almost by chance I have discovered a remedy for certain affections of the tongue which will prove most valuable in speedily removing them. Sir James Paget, in his Clinical Lectures and Essays, says that a most useful local treatment of gouty psoriasis of the tongue is to paint the white patches with a saturated or very strong solution of chromic acid. Although the out-patient department of a large hospital does not afford a large number of cases of gouty psoriasis, it does bring many patients suffering from affections of the tongue, and among them a fair proportion of cases of chronic superficial glossitis, most of which are due to excessive smoking and drinking, often combined with syphilis. On some of these cases of glossitis I determined to try chromic acid in solution, and during June (1881) treated two patients with a ten-grain solution in water, which was ordered to be painted on the sore areas of the tongue three or four times every day. Both patients certainly improved under this treatment; indeed, one of them, who had been previously treated with glycerine of tannic acid and other remedies during several months without avail, was so much relieved in the week by the acid that he was lost sight of until the following October, when a fresh attack of inflammation brought him back to the hospital and discovered to us the reason of his disappearance.

At the same time that this man was attending the hospital I had also under my care a man (J. B.) suffering from obstinate

secondary syphilitic ulcers of the borders of the tongue, small but deep and jagged, and from ulceration of the inside of the left cheek. During April and May he took hyd. c. cret., until, at the end of two months, his gums became a little sore. Iodide of potassium was then substituted for the mercury and continued in doses of from ten to twenty grains until July, when the dose was diminished to five grains, and a dram of liq. hyd. bichlor. was administered. He took this medicine regularly until October 17 (1881), but really without any improvement in the ulcers, which, indeed, looked worse and were larger than when I first had seen them. He was put on chromic acid solution merely as an experiment, and without much hope of affording him relief. But a week later (October 24) he returned with the ulcers almost completely healed. On October 20, before the result of treatment on J. B. was known, I ordered a chromic acid solution for a man (T. L.) who had a number of flat mucous tubercles, due to secondary syphilis, on the right border of his tongue. He had been treated with hyd. c. cret. gr. iii. from June until October, and with powdered calomel and other local applications without the slightest benefit. On October 27 his tongue was almost well, and in the course of between two and three weeks scarcely any trace of the tubercles remained.

Since that time I have employed chromic acid in the treatment of several kinds of inflammatory conditions of the tongue, and in certain cases with most marked success. I have notes of twenty-seven patients, twenty of whom have been cured or greatly relieved, seven have received little or no benefit. The seven cases were either of chronic superficial glossitis, or of *tertiary* syphilis. The twenty patients include seven suffering from chronic superficial glossitis, and thirteen from various *secondary* syphilitic affections. From these cases the following conclusions may be drawn: Chromic acid cures with marvellous rapidity *secondary* affections, ulcers, mucous tubercles, condylomata. It produces no appreciable effect on *tertiary* affections, gummata, extensive ulcers, tubercular syphilides. Some cases of chronic superficial glossitis, where slight ulceration and renewed inflammation has occurred, improve quickly under its influence. Other cases of glossitis, in which the tongue surface is attacked by a fresh inflammation of great severity, so far from improving, appear actually to be rendered worse by chromic acid. Glycerine of boracic acid and soothing remedies are more suitable for such conditions.

Fortunately, the secondary syphilitic affections are those for which some rapid remedy is most needed, for the tertiary affections generally yield rapidly to large doses of iodide of potassium or to mercury, and they are probably not at all con-

tagious, as the secondary affections undoubtedly are. A patient, whose tongue and lips are quickly cured of secondary syphilis, is not only relieved, so far as he himself is concerned, but he ceases to be a source of contagion to those with whom he associates, a source of contagion often the more dangerous in that neither he nor those around him are conscious of the danger. The only objection to the rapid curing of these affections, in hospital patients especially, is that they are unwilling, after the local mischief is relieved, to continue the long course of constitutional treatment which is necessary for the complete cure of their syphilis. Many of them withdraw themselves at once from further treatment.

In contradiction to the statement previously made that tertiary syphilis is not benefited by chromic acid, I may mention one case in which an excellent result followed its employment. A medical man of my acquaintance called on me last June to ask whether I thought that some small, ragged, and deep syphilitic ulcerations of the tip and borders of his tongue were likely to be transformed into epithelioma, of which he had a great and natural horror. The primary attack of syphilis had occurred at least ten or twelve years previously; it had been treated during many months with mercury and iodide of potassium. The ulceration of the tongue had been a source of great annoyance and distress to him during many months, although he had been under the care of several of the most eminent surgeons in London, one of them especially skilled in general surgery and syphilis. Bicyanide of mercury and caustics, with various constitutional measures, had been employed, but without avail, and he came to ask me—not to cure him, for he believed he was incurable—but whether he would die of cancer of the tongue. By my advice he took two and a half grains of hyd. c. cret. daily, and painted his ulcers with a solution of chromic acid. I neither saw nor heard anything of him until November, when he came to show me his tongue, quite healed and much less scarred than could have been expected. Under the treatment it had quickly improved, and in less than a month was well. Six weeks later a second outbreak had occurred, which had given way to treatment in two or three weeks. There can be very little question that in this case the mercury had taken an important share in effecting a cure, but I do not think the mercury without the chromic acid would have availed, and certainly not with such rapidity. It may be that the acid acted well in this instance because the ulcers did not resemble typical tertiary ulcers, but rather the ulcers which attack the tongue in the later period of secondary syphilis.

The strength in which the chromic acid has been almost invariably employed has been ten grains to one ounce of water.

In a few instances fifteen grains have been ordered. The patient has been told to paint the diseased portions of the tongue three or four times a day with a camel's-hair brush dipped in the solution. Pain or discomfort from the application has seldom been complained of; and even if there has been a little smarting at first, this has cheerfully been borne on account of the relief which the lotion has afforded.

Of the precise mode of action of the acid I cannot speak with certainty, partly because I have not been able to remove the healing parts in order to examine them, partly because an interval of several days, often a week, has elapsed between the dates of my seeing the patient. But I have observed that raised areas quickly subside until they reach the level of the normal tongue, and that the little red areola by which they are surrounded disappears and gives way to the normal colour of the mucous membrane. From this, perhaps, it may be assumed that the epithelium of the raised areas (mucous tubercles and condylomata) shrinks or is cast off, and that the vessels contract under the influence of the acid. Its action on ulcers is less apparent.

I hope at some future time to be able to afford more definite information on this point, and also to be able to describe the effect of chromic acid on syphilitic affections of parts other than the tongue and lips. At present I can only say that it is very useful in secondary affections of the tonsils and the palate.—*Practitioner, March*, 1883, p. 175.

47.—ON THE TREATMENT OF ENLARGED TONSILS.

By Gordon Holmes, M.D., London.

Excision.—That excision is the most fitting remedy for enlarged tonsils is a proposition from which few authorities or none would now be found to dissent. The theory, however, that removal of the tonsils has some adverse effect on the generative system has gained some currency amongst practitioners. Those who practise the throat speciality will scarcely attach any importance to this hypothesis, as they have the opportunity of observing numerous cases of females who have borne large families, although the tonsils were excised in youth.

The condition of the tonsils at the time of operating is a point which deserves attention. As a rule, when the patients first present themselves, the organs are in a state of acute or subacute inflammation, which is the immediate cause of treatment being sought. Under these circumstances, some practitioners are in the habit of operating at once, whilst others wait until the tonsils have regained their ordinary condition

of indolent chronic enlargement. In order to decide this question, the arguments to be drawn from clinical observation are rather in favour of delay. It is no doubt well for the patient to be relieved at the soonest from the diseased glands, and the excision, with the attendant hemorrhage, may abridge, or render abortive an attack of quinsy. If, however, the tonsillitis is very acute, the pain of the operation may be excruciating, and it may even be difficult to open the mouth wide enough for the accurate introduction of the instruments. But the most important consideration is that after the inflammation has subsided, and a few weeks have been employed in general tonic and local astringent treatment, the tonsils may return to nearly their natural size. Of course should the history of the case indicate clearly a permanent hypertrophy, we need not hesitate to operate immediately when other circumstances are favourable. It is, indeed, by no means uncommon to see even cases of mild subacute or chronic tonsillitis, in which a considerable swelling undergoes spontaneous involution in the course of a month or two, according as the general health improves. Such instances are very likely to mislead the practitioner into the belief that a cure has been wrought by the aid of some really impotent local application. It is also interesting to observe that in a series of such attacks, where the subsequent involution is always less and less complete, we can perceive the origin of chronic tonsillar hypertrophy. And most probably there is no other way in which the disease arises, unless in those cases where the enlargement is apparently congenital.

With respect to the method of operating, the tonsillotome has almost superseded the bistoury and tenaculum, owing to the ease and rapidity with which it can be used, and the little suffering it causes the patient. The rare case of a tonsillar calculus alone remains in which the tonsillotome is inapplicable. The instrument known as Physick's is preferred by some, that of Fahnstock by others, the majority; both models being of American invention. Physick's pattern has the advantage of allowing more force to be applied in fixing it firmly on the tonsil, as it is grasped by a stout handle, whilst the convex cutting blade can also be pressed down with great power by the thumb. Fahnstock's, on the other hand, is an instrument to be employed with dexterity rather than force, as it is manipulated by the thumb and first two fingers only, each of which is accommodated with a separate ring. It is, however, a much surer instrument than Physick's, which often fails to excise the tonsil, even in the hands of the most practised operators. This is mainly due to the aptness of the convex blade, which enters at one point only, to press the tonsil outwards, and glide over

or merely lacerate its surface, even when sharpened to perfection. The tonsil is, in fact, often very grisly, and requires to be jammed very firmly against the blade in order to be cut directly through. But the lunated edge of Fahnstock's cutting ring is much less likely to swerve from the straight course, as it begins by being in contact with the whole surface to be incised, whilst the organ is steadied and drawn into the tonsillotome by the transfixing prongs. As Fahnstock's instrument is therefore almost certain never to miss the tonsil, it is more under the command of the operator, so that a larger or smaller portion can be removed according to the requirements of the case; whereas in using Physick's, owing to the necessity of fixing it as deeply and firmly as possible over the tonsil, the amount to be taken must mainly be left to chance. For the practitioner who seldom has occasion to excise the tonsils, Fahnstock's is decidedly the model to be recommended. In all cases it is advisable to support the tonsil during the operation from the outside of the neck, either by the hands of an assistant or by grasping the throat in a fork formed by the left thumb and forefinger of the operator.

As regards hemorrhage after tonsillotomy, it is usually very trifling and ceases spontaneously in a few minutes, or after gurgling with cold water. In rare cases (probably about 1 per cent.) it is troublesome and must be controlled by ice and strong styptics. Considered in this relation the tonsillotome is safer than the bistoury, as the dragging of the tenaculum displaces the parts so that the knife may inadvertently cut deeper than intended by the operator.

Caustics.—Although excision is the standard remedy, cases are constantly met with in which the patient or his relatives, if a child, steadfastly refuse to permit the performance of the operation. Frequently also it happens that the enlargement is not so pronounced as to render the excision strictly necessary. It is of great importance therefore that we should be in possession of some mild measures, which, aided by time, can effect a reduction of the tonsils. For the purpose astringents (such as strong solutions of perchloride of iron, chloride of zinc, &c.) or caustics are our only resource; but the former, however useful in promoting spontaneous resolution in cases of temporary subacute engorgement, are totally powerless in the face of a true hypertrophy. Hence the only means that can be adopted with any clear prospect of success is to destroy small portions of the tonsils in slow succession by repeated cauterisations carried on for a lengthened period. This object is generally carried out by the application of solid nitrate of silver, chloride of zinc (in the stick), Vienna paste, London paste, or the galvano-caustic to a limited area of the surface of the tonsil every

two or three days. Of these remedies the nitrate of silver is very valuable when the superficial substance of the gland is in a softened and raw state, as it can be rubbed freely over the whole organ, usually without causing any suffering to the patient; but it is of slight avail in reducing the volume of the toughened mass of connective tissue which generally constitutes the bulk of the permanently enlarged tonsil. Nor can it be said that the stronger caustics are much more potent in this way, for they can only be used in this position so as to destroy very minute portions at a time, and hence the progress is mostly so slow and tedious that few patients will persevere until positive results have been attained. If any exception can be made to this statement, it must be in favour of the galvano-caustic (a small loop of wire raised to a white heat by electricity as soon as placed in contact with the surface to be cauterised), for it kills immediately the part touched and leaves a clean ulcer, which soon heals almost unfelt by the patient. On the other hand, the London paste, &c., often give rise to considerable congestion round the sloughing point, attended with aching and pain on swallowing for many hours.

There is, however, another method, very much more effective, of applying the common caustics to the tonsil, which appears to have remained hitherto unnoticed. The tonsil, as the anatomist knows, is permeated by several rather large channels around which the follicles are collected, opening on the pharyngeal side of the gland, whence its characteristic cribriform aspect. Their orifices, about seven to fifteen in number, are sufficiently evident to be counted on the healthy tonsil *in situ*, whilst in the hypertrophied condition these lacunæ increase greatly in calibre and depth, and can be ascertained by a probe to vary from one-eighth of an inch to half an inch in length, with a diameter capable of admitting a style of ordinary size. These observations, then, afford a valuable indication for treatment: for through these natural canals a way lies open for us to attack the heart of the gland in a most efficacious manner with our caustics. Thin, pointed sticks of nitrate of silver or chloride of zinc can easily be pressed into the lacunæ and worked round for a few seconds. Small sloughs are thus formed which are soon discharged, and in the progress of this treatment the tonsils are hollowed out in one direction whilst being contracted into much smaller bulk by the subsequent cicatrisation in another. Two or three channels in each tonsil can be cauterised daily or on alternate days, and we can thus act on a comparatively large surface whilst causing but slight external soreness and little or no suffering to the patient. In practising this method, although the stronger caustics may be used, I do not think it will be necessary to have recourse to anything more

potent than nitrate of silver, which acts much more effectually on the more tender internal structures of the tonsil than when applied to the comparatively callous pharyngeal surface.—*Lancet, Nov.* 11, 1882, *p.* 798.

48.—ON AIR-INFLATION OF THE BOWEL AS A RULE IN THE OPERATION OF LEFT LUMBAR COLOTOMY.

By Edward Lund, F.R.C.S., Professor of Surgery in the Owen's College, &c., Manchester.

At a recent meeting of the Royal Medical and Chirurgical Society, I had the honour to exhibit some instruments which I have invented for facilitating the operation of left lumbar colotomy. These have since been perfected by Messrs. Mayer and Meltzer, and are shown in the woodcuts, as follows :—

In Fig. 1 A is an air-syringe and rectum tube for inflating the colon during the operation, and rendering it more visible and prominent at the bottom of the wound; and such an instrument will be found useful for air-inflation of the bowel in cases of suspected intussusception, or obstructed intestine from other displacements. The merit of the invention consists in a particular mode of securing an air-tight contact around the margin of the anus, by the use of a hollow elastic ring, E, placed over the tube, which is compressed and flattened against the shoulder, B, on the handle, C, when firmly pressed against the part by an assistant. This method of preventing the return of the air as it is pumped into the bowel is more effective than anything of the nature of a plug or tampon introduced within the rectum, even if it be carefully adjusted to the size of that cavity, for the air so injected is sure to escape by the side of the plug, the anus and rectum being immensely expansible, and yielding to pressure from within almost indefinitely. This dilatability of the anal aperture is proved in the manual exploration of the rectum, occasionally resorted to for diagnostic purposes. With the apparatus here described, when the hollow ring is compressed, the central hole in it is diminished

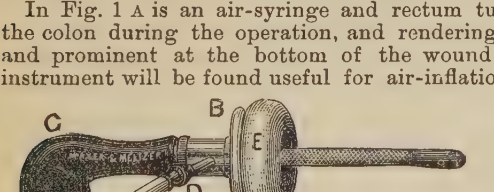


FIG. I.

in size, the skin around the anus, to which the indiarubber clings with great tenacity, is drawn inwards towards this centre, and the tightness of the air-joint thus formed can be well sustained. The air enters the tube through the side of the handle at the point D, and, if the conditions of the rectum will permit, a long narrow tube, F, should be used and passed high up into the bowel, so that the air may be delivered into the sigmoid flexure or the colon; but if there is a growth in the rectum or much contraction which obstructs the passage, a shorter tube must be employed and time allowed for the air to pass upwards through the diseased mass. Under any circumstances some time elapses before the colon can be fully expanded by this air-inflation, the circular fibres of the bowel resisting its dilatation in spite of the anæsthetic condition of the patient. As soon, however, as this resistance has been overcome, the colon will be seen to increase in volume and to fill the wound and roll forward towards the umbilicus, into just the position most favourable for the safe introduction of sutures and the section of the bowel on its posterior aspect.

Fig. 2 illustrates a form of armed mounted needle, of such a shape and curvature as to readily pierce the colon without allowing the escape of the air within. If its point be applied to the distended bowel at right angles to its surface, and pressed onwards until it has pierced all the coats of the intestine, a semi-rotatory movement of the handle will then cause the point to emerge at a convenient distance, without danger of the needle passing between the layers of the bowel in the loose tissue external to its mucous membrane. The needle is next threaded with a long silk suture and withdrawn, and the shaft of the needle being very strong and of a conical form, as it is introduced the puncture is plugged by the advance of the thicker portion, and although on its withdrawal the direction of the cone will be reversed, yet even then very little air will escape.

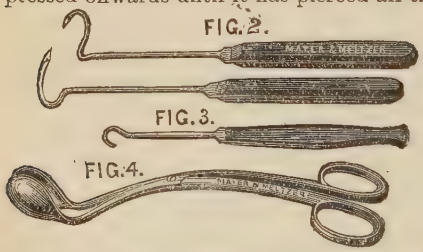


Fig. 3 shows a blunt hook in handle, which, introduced into the colon through the incision in the bowel, will be found useful for seizing the suture-thread within the colon and dragging it out, a proceeding which is sometimes a cause of delay at this stage of the operation.

Fig. 4 shows a pair of curved forceps with spoon-shaped ends,

very serviceable in the after-treatment of colotomy cases for the removal of the hardened masses of fæces which are apt to accumulate below and above the artificial opening. These forceps can be passed through the wound without pain to the patient, and the fæces removed with great ease. In the operation of colotomy, after the preliminary incisions have been made, I am very careful to smear freely over the cut surfaces an abundance of carbolated oil, so that when the bowel is opened and the fæces escape, the divided surfaces of the wound may be mechanically protected and future irritation avoided. I also employ vaseline and eucalyptus (one-tenth) as the permanent dressing, for I have found it to decrease in a marked degree, if not to entirely destroy, the unpleasant odour of the feculent discharges. —*Lancet, April* 7, 1883, *p.* 588.

49.—RADICAL CURE OF HERNIA BY REMOVAL OF SAC AND STITCHING TOGETHER THE PILLARS OF THE RING.

By W. Mitchell Banks, F.R.C.S., Surgeon to the Liverpool Royal Infirmary.

Of late years there has been a revival of one of the oldest methods of cure—that by removing the hernial sac. That surgical bogie, the peritoneum, has been exposed; and we now know that, if not poisoned, it is a very tractable and inoffensive membrane. Indeed, even in cases of death after operation for strangulated hernia, it is very rarely that peritonitis kills the patient. Moreover, the confidence inspired by Listerism has made an astounding change in our views of the surgery of serous membranes; and to Lister is due the credit of having been early in the field, even in the department of curing hernia. So far back as 1871, he related two cases to the British Medical Association, at Plymouth, which stimulated many surgeons to follow in his footsteps. Among the most indefatigable has been Prof. Annandale, who, in December 1880, contributed an admirable article on the radical treatment of rupture to the Edinburgh Med. Journal. He there enumerates four operations that have been performed upon the sac: 1. Ligature of the neck of the sac alone. 2. Ligature of the neck of the sac, with invagination of the ligatured sac into the abdominal opening. 3. Ligature of the neck of the sac, and excision of the sac below the ligature. 4. Ligature of the neck of the sac, with excision of the sac and stitching together the margins of the abdominal opening. He says: "Having used all these methods, I have no hesitation, from my experience, in giving the preference to the fourth plan." This is the plan which I have myself adopted; and I have no hesitation in saying that it is the best plan yet found out, and the one which will, in a short time, prove by far

the most popular. It is applicable to every case of hernia that requires to be interfered with, and every surgeon can do it.

In performing it, I use thorough antiseptic treatment, and make a point of having the pubes, and parts around the anus, most carefully shaved. In an inguinal hernia, the incision should commence at least an inch above the upper margin of the external ring, so that plenty of room may be given thoroughly to clear the pillars for the stitching. The sac is next freed from the surrounding tissues, and this is often much more troublesome than might be imagined. One is almost always tempted to think that it has been reached long before it really has; so that frequently, after a considerable amount of stripping has been done, it is found that it is not the sac at all that is being cleared, and the process has to be done over again. It is this mauling of the loose cellular tissue of the scrotum that gives rise to nearly all the trouble that occurs in the way of suppuration. The sac ought to be fairly reached before any stripping of it is done. Another point is that, in the case of an old sac, the lowest point is intimately adherent to the tunica vaginalis; and, if it be roughly pulled upon, the testicle enclosed in the tunica comes bodily out of the scrotum —not a very serious matter, it is true, but unpleasant to look at. The sac having been cleared, its contents are pressed up into the abdomen. When it is thin, there is no difficulty in making sure that it has been completely emptied; but, if there be the slightest doubt, it should be slit up and its interior examined. Adherent omentum, if in small quantity, I separate carefully, tie with catgut in one piece, and cut off; if in large mass, I split it up into two or three portions, and ligature with carbolised silk to ensure a good knot that will not slip. One cannot be too careful about the securing of the omental stump before it is finally pushed into the abdomen, and every drop of bleeding should have ceased, both from the omentum and from the neck of the sac, before the next proceeding. This consists in pulling the sac well down, and tying it as high up as possible. I use two ligatures of strong catgut, as that material is apt to be treacherous. In case anything should happen to one, the other is there. With a curved needle, armed with strong silver wire, I next pull together the pillars of the external ring, leaving only room for the spermatic cord at its lowest part. Two or three stitches suffice. These are cut short off, and left *in situ*. A clean carbolised sponge, put beneath antiseptic gauze for the first twenty-four hours, makes the best dressing.

The only point of novelty for which I take any credit to myself in this operation (and very likely others have done it as well as myself) is the use of strong silver wire to draw

together the pillars of the ring, which is left permanently in position. Catgut I distrust, where there is any strain upon it; it yields too soon. Silver-wire seems to bury itself so harmlessly, that I cannot see any objection to it; while it must of necessity hold the pillars together for a considerable time, until they are well agglutinated. I tried magnesium wire once, with the idea of its oxidising and disappearing, but it was too brittle; and, after all, silver wire is just as innocuous. In the case of femoral hernia, I content myself with mere removal of the sac, as the introduction of sutures to pull down Poupart's ligament to Gimbernat's would be very troublesome, while there is not the same necessity for suture that there is in inguinal hernia. The femoral rupture is very rarely so large as the inguinal, and, consequently, the femoral ring is not dilated so enormously as the inguinal canal is, while its walls are more rigid and unyielding.

At the present moment, I believe that the choice of an operation for the radical cure of hernia, lies between Mr. Spanton's method and that which I am now advocating. Quite recently, in the British Med. Journal, that gentleman has published a record of thirty-four cases which deeply interested me, as affording an excellent basis of comparison between the two plans. In the first place, looking at the ages of the persons operated upon, of the thirty-four, twenty-two were children below 11 years of age, and the remaining twelve were young persons between the ages of 13 and 27. Turning to my thirty cases, it will be found that twenty-two were between 30 and 64 years of age, and six between 18 and 30. Only once was the operation done upon a child. Mr. Spanton very candidly says: "The most suitable cases are, I think, those occurring among the young." But many may question whether the young need to be operated upon at all, except in very rare cases. Personally, I must confess that I have a strong belief that, in children under ten years of age, a well-fitting truss, worn constantly to the age of fifteen, will cure the great majority of their ruptures. Nevertheless, there are cases, even in children, which demand operation; and to these Mr. Spanton's operation seems admirably adapted, inasmuch as the patients are always thin, with their fibre firm and in good condition, and seldom troubled with cough. To keep the hernia up, the restraining force required is seldom great, and a moderate amount of irritation will suffice to close the neck of the sac, and so effect a cure.

With regard to the introduction of the radical cure, as part of the ordinary operation for strangulated hernia, I think it will mark an important epoch in the history of that operation; and that, in future, the description, in place of commencing

with directions to cut down upon and open the sac, will commence with directions to dissect the sac clear, so that it may be removed when its contents are restored to the abdominal cavity.

In instituting a comparison between Mr. Spanton's operation and the one under consideration, I should not for a moment desire to be considered as undervaluing the former. All I wish to show is that, compared with what may be called the sac operation, its capabilities are limited. Nevertheless, I regard it as a most ingenious device; and I firmly believe Mr. Spanton in all that he says with regard to its success in the cases in which he has tried it. I have not performed it myself, because I have been so interested in the other, that I have only looked out for those serious cases to which it is specially adapted. But I think that, if a parent thought his son would be the better of having his hernia radically cured while a boy, I would recommend Mr. Spanton's operation for the lad as the safest, and as probably equally effectual. Both operations have their sphere, and both will become popular in due time—for the simple reason that they are easy and can be done by anybody. I trust we may both be successful in exciting, in the minds of our fellow practitioners, a desire to do something for the relief of a vast body of suffering men and women, whose lives, by reason of rupture, are often rendered very miserable and useless, and, not unfrequently, put in deadly peril.

The following are the chief conclusions at which I have arrived.

1. For simple inguinal hernia in boys, where the sac and its contents are reducible, Mr. Spanton's operation seems highly suitable.

2. The sac operation is applicable to herniæ of all conditions, and specially to those reducible ruptures where there is adherent omentum in the sac.

3. As far as my thirty cases go, it is shown not to be a dangerous operation; while its results from a curative or a remedial point of view are most satisfactory.

4. Radical cure should form a necessary part of all operations for strangulated hernia.—*British Medical Journal, Nov.* 18, 1882, *p.* 985.

50.—CASE OF RADICAL CURE OF INGUINAL HERNIA.

By James Whitson, M.D., &c., Surgeon to the Dispensary of Anderson's College, Glasgow.

Approximation of the Pillars of the Ring by Means of Chromicised Catgut—Recovery.—John McB., aged fifty-six, labourer, was admitted on October 18, 1882, to the Glasgow Royal

Infirmary, suffering from what was supposed to be a strangulated inguinal hernia, which, however, Mr. Greenhill, house-surgeon, succeeded in reducing, without difficulty, the ring being a wide one, and the distance, in consequence, between its pillars being very considerable. The patient having afterwards expressed a desire to be relieved of his infirmity, which was a constant source of annoyance as well as danger to him, I resolved to perform an operation for the radical cure of hernia; but, before doing so, I thought it expedient to give him a few days' rest in bed, so that his bowels might be carefully regulated, and any shock which he had suffered from on admission might have had time to pass off.

On October 27 the patient was put under chloroform, the pubes carefully shaved, and, with the kind assistance of my friend, Mr. Clark, I proceeded to operate by first of all transfixing a fold of skin over the inguinal ring, and cutting outwards. The various structures between the skin and the mouth of the canal were successively divided on a director until the sac was reached, when it was opened into, and a large portion of it removed. Wood's needle, threaded with the strongest chromicised catgut, was then passed through the upper and outer side of the ring, carried over towards the inner pillar, and when brought into contact with the skin on that side, the latter was pulled away in the direction of the mesian line, so as to permit of the exit of the point of the needle at the opening already made. Three separate sutures of catgut were introduced in this way; and the cut edges of the sac were stitched together with moderately sized gut. The pillars of the ring were then firmly approximated by tightening the sutures, and securing them with reef-knots. A decalcified drainage tube, threaded with horse-hair, was inserted into the wound, the margins of which were brought together with two button sutures, and accurately apposed by means of several stitches of fine gut. Protective plaster and gauze were next applied in the usual way, and an elastic bandage assisted in keeping the whole in excellent position. The dressings were changed on the second day, when the hair was withdrawn from the drainage-tube, and union by first intention had taken place along the whole line of incision, with the exception of the lowest point, where a necessary aperture for the exit of the discharge remained. It is needless to give further particulars of the progress of the case, as the wound followed a truly aseptic course, and the patient made an excellent recovery. He was dismissed, cured, on December 2, 1882.

Up to the present time surgeons have generally been in the habit of using silver wire, in order to obtain a permanent approximation of the pillars of the ring; and Mitchell Banks,

of Liverpool (*vide* preceding article), speaks of this material burying itself harmlessly in the tissues; but it would seem to me evident that a ring of thick wire can hardly be present in the body without causing inconvenience in its own immediate neighbourhood, or, what is equally objectionable, eating its way gradually from within outwards. The same writer, as well as Mr. John Wood, of London, both say that they distrust catgut, as it yields too soon; and, beyond a doubt, the great bulk of the so-called "chromicised catgut" of the shops will soften and become absorbed in the course of two or three days. Owing to this, disappointment in the results of many operations has followed as a natural consequence, not from any inherent defect in the material, but from its being insufficiently hardened, either by a too brief immersion in the chromic acid solution, or by its being steeped in a mixture improperly composed, and therefore not adapted for imparting the requisite degree of durability. If prepared, however, by the process recommended by Macewen, and kept in solution for a week, it will be found thoroughly reliable, and capable of resisting the action of the tissues for a fortnight. Catgut, in this capacity, has many advantages over wire. It possesses greater pliancy, and adapts itself much more readily to the position of the parts with which it comes in contact. A firmer and better knot can be tied with it than by means of a metallic suture, and one which takes a closer and more equable grasp of the structures which it surrounds; and on account of its bland and innocuous qualities it never causes irritation, while at the same time, from its ultimate absorption, it cannot possibly produce after-discomfort.

Macewen's method of preparing chromicised catgut is as follows: One part of chromic acid is first added to five of water. One part of the solution thus made is then mixed with five of glycerine, and if the gut is steeped in this for a week, it will be found capable of resisting the action of the tissues for a fortnight.—*Medical Times and Gazette, Jan.* 27, 1883, *p.* 91.

51.—ON THE TREATMENT OF FISTULA IN ANO.

By Edward Hamilton, F.R.C.S.I., Surgeon to Steevens' Hospital, Dublin.

The internal opening of rectal fistula has been the subject of much controversy—first, as to the frequency of its existence; secondly, as to its position; and, thirdly, as to the mode of its production. So far as statistics can shed light on the question we have the authority of Ribes, who gives us a report of eighty cases which he had the opportunity of examining after death. In all these subjects he found that the internal opening could be demonstrated. The frequency of this occurrence led him to

look for the cause of fistula in some morbid condition of the mucous membrane; and he fixed upon the suppuration of an internal pile as, in ninety-nine cases out of a hundred, causing the disease. Mr. Lane, of St. Mark's Hospital, records a series of cases submitted to operation, of which forty were complete, twenty were blind external, and two blind internal. In doubtful cases the injection of milk was used to determine the presence of the internal opening. Sir B. Brodie also expresses the opinion that fistula commences as a small ulcer on the lining membrane; and there can be no doubt that such an ulcer, an internal pile, or the lodgment of a foreign body, a splinter of bone, the seeds of fruit, the core of an apple, becoming entangled in some of the numerous folds of the rectum, may cause inflammation and ulceration; matter collecting in the small depression thus formed makes its way slowly but steadily into the ischio-rectal space, ultimately pursuing its course to the surface of the skin. And it is a clinical fact that, in a very large majority of those affected with this disease whom we are called upon to treat, either in hospital or in private practice, the fistula is complete; but we must remember that they are, for the most part, chronic cases in which time has been afforded for the extension of ulceration, which commenced as an abscess, having no opening either on the mucous membrane or the skin, but ultimately presenting both one and the other. A consideration of more practical importance than either the relative frequency or the cause of the internal opening is *its situation* when it does exist. Ribes states positively that in none of the eighty cases which he examined was the opening higher than five or six lines from the verge of the anus; and in many of them it was not so high, being just at the juncture of the skin and mucous membrane, so as to be visible on slight protrusion of the bowel. Velpeau tabulates the result of thirty-five *post-mortem* examinations as follows:—In four, the internal opening was one inch and a-half from the anus; in one, it was three inches; and in thirty a few lines only from the anal outlet.

This pathological fact teaches us that it is not necessary, as a rule, to make any deep gash in the walls of the intestine, and that surgeons frequently cause unnecessary pain and suffering in searching for the opening higher than it really is. The result of my own observation in the dissecting-room, where we meet with more numerous examples of fistula than in our *post-mortem* theatres, is fully in accord with the statements of Ribes, Larrey, and Velpeau. There appears to me to be a spot, definable with anatomical precision, where the deep opening is almost constantly to be found. I mean the crevice between the lower margin of the deep and the inner border of the

superficial sphincter. Along this ring the wall of the intestine is especially thin and readily permits of perforation, yielding to the pressure of the matter. Next in point of frequency is at the upper margin of the deep sphincter. I have rarely seen it higher than this, unless where there is a second opening into the rectum. There is an important clinical fact which must not be lost sight of, as it has caused considerable confusion—it is this: the height of the intestinal opening does not definitely fix the height to which the tunnel may extend. We find infinite variation in this respect. The tract may wander away from the bowel far above this point, and, as we see frequently enough, may pass round the gut like a horse-shoe. Hence I think it necessary to include "*complex fistula*" among the varieties of the disease which the student should carry in his mind.

It must never be forgotten that a sinus may exist at the side of the rectum, and may have no further relation to it. It may be evidence of formidable disease higher up in the canal; it may originate from disease of the spine or pelvic bones, psoas abscess, or disease of the urinary organs; it may result from stricture high up, remote from the anus. The connexion between these two conditions, stricture and fistula, must never be lost sight of, and it is remarkable how different the relation is here when contrasted with similar affections of the urethra, where we find the fistulous tract running up to the passage behind the stricture, whereas in stricture of the rectum the fistulæ open, as a rule, below the constriction.

The *symptoms of fistula* are usually expressive enough. There is some pain and uneasiness during and after defecation, a flow of mucus from the bowel, and consequent excoriation of the adjoining surface from the constant moisture; flatus may escape through the opening; occasionally a bubble of air may occupy it. Should such indications exist after an attack of inflammation, with rigors, they surely point to the existence of rectal fistula. These symptoms are very often attributed by patients to piles. You must, therefore, never be satisfied until you have made a careful examination of the parts. What more melancholy example can we have of surgical imbecility than the man who sits down and by rule of thumb prescribes sulphur electuary and gall ointment in such a case.

The information to be gained by *exploring the bowel with the finger* is simply invaluable, and when conducted with gentleness and ordinary care causes very slight pain or uneasiness. The finger, well oiled, and the groove of the nail filled with soap, should be slowly introduced by a gradual boring movement, and passed fully into the bowel. The act of doing so will give you much valuable information.

A resisting spasmodic sphincter indicates irritability caused by ulcer; the lax and patulous anus, with brickdust discharge, indicates advanced malignant disease; soft projecting folds tell of internal piles; a nodulated irregularity should lead you to seek high up for simple stricture; a circular depression, with indurated margin, tender on pressure, marks the position of the ulcer of the rectum; a small thickened point the internal opening of fistula. As a rule, this opening is soft, as if ruptured. It is said to be more frequently hard and thickened in phthisical subjects. The path of the rectum is now frequently resorted to for the purpose of clearing up the diagnosis of disease of the pelvic cavity, and as an aid to the use of the catheter in difficult cases—it is as the sounding-lead to the sailor, enabling him to avoid unseen shoals and hidden rocks—guiding the point of the instrument through the narrow channel of the urethra into the bladder. In this instance, however, you gain something more, for the presence of the finger distends the sphincter and tires out all the perineal muscles. At first we find every one of them thrown into commotion, every attempt to advance the instrument causes fresh spasm, until at length the muscles are fatigued, and the chief source of trouble and difficulty completely overcome.

Should still further examination be required we must use the *speculum ani*. The modifications of this instrument are very numerous—some, made of strong wire, are exceedingly simple, and others complex enough to delight the heart of the most artistic mechanist, but in practice utterly worthless, painful to the patient, and unsatisfactory to the surgeon. In the use of instruments always select the simplest which will accomplish the object; they are, as a rule, the most efficient. That which you have seen in use most generally in this hospital, both for exploring the rectum and operating for fistula, is a simple lithotomy gouget. It is shaped for slipping along the finger, and by altering its position you can examine in detail every portion of the rectal wall. The little lamp used in aural surgery will be a valuable help in the absence of sunlight. By using two such specula, one on either side of the finger, you can dilate the anal opening to a considerable extent and get a complete view of two opposite sections of the bowel. By changing the position of instruments the remaining segments may be brought into view. You have on the table a set of specula which act on this principle. They are figured in "Bryant's Surgery." I have in this way used the smaller end of Sims' speculum for vesico-vaginal fistula. Test each of these instruments for yourself, and employ that which gives you the most satisfactory view of the parts.

We cannot leave the subject of exploring the rectum without

alluding to the practice of *introducing the entire hand into the bowel*, which is now a recognised surgical proceeding. I need hardly say the practice is not adapted to cases of disease of the rectum itself—in fact, such a condition would be a strong indication against undertaking it at all. In a state of anæsthesia, with great care and patience, the entire hand, more especially in the female, may be passed into the bowel, so as to examine parts above the brim of the pelvis, the kidney, the aorta, tumours of different kinds. This operation should not be lightly undertaken, as it is by no means free from danger, the bowel may be lacerated, and diffuse inflammation of the pelvic areolar tissue may be set up. It requires the utmost care and gentleness. A case related by Nolet, surgeon to the King of France, is one of the earliest on record of this operation. A monk, in order to cure a colic, introduced a bottle of Rhenish wine, which slipped from his grasp, and became lodged in the bowel. A "wise woman" attempted to introduce her hand, but failed. This was subsequently accomplished by a young lad trained to it, and the bottle was removed. Thiandière relates a case where he introduced finger after finger until the entire hand was passed into the gut, for the removal of a forked stick, which had been designedly pushed into it.

In order to facilitate these explorations of the rectum, Prof. Van Buren, of Bellevue Hospital, strongly advocates the *forcible dilatation of the sphincter*, so as to induce temporary paralysis, rendering the anus soft and unresisting. It is best accomplished by inserting the thumbs into the bowel, while the palms are placed upon the buttock. We have thus enormous power of divarication. I need scarcely say such a proceeding demands the full effects of anæsthetics. You may remember, in a case of hemorrhoids on which we operated lately, the patient, a female, declined to have ether, and endured the operation without its aid with wonderful fortitude.

Surgeons differ as to what is the most suitable *position in which to place the patient for this examination or for operation*—the prone, the supine, or on the left side. I give the preference myself to the last. The patient being placed on the left side, the thigh of that side should be secured to the table by a strap or broad flannel band; the right knee should be flexed to the utmost, and secured in this position by a strap or band round the neck of the patient. Many surgeons prefer the prone position and the use of Clover's crutch, as for lithotomy.

We have now to discuss a most interesting and important point in pathology—*the connexion* which, from the earliest records of surgery, has been *alleged to exist between pulmonary consumption and rectal fistula*, and the hypothesis that they mutually re-act one upon the other. We must start with the

admitted fact that these two conditions frequently co-exist, and I think we may fairly ask to assume that the same low state of the system which predisposes to one will aid in the development of the other. When we advance beyond these two postulates, we find the ground becoming more uncertain; we get into the region of dogmatic assertion, and find ourselves hampered with the trammels of long-established authority tacitly recognised and almost unquestioned. It has long been a canon of surgery that you should never cut a fistula if you have any suspicion of the existence of phthisis; that the discharge from it acts as a derivative—a safety-valve which keeps in check the lung trouble, that this will run its course with intensified rapidity if the purulent secretion be interrupted. Modern surgeons are inclined to accept this dictum *cum grano salis*, and might not reason suggest the question—Is a man with a weakly constitution better off with two cachectic diseases than with only one? and certainly we can easily anticipate the answer which common sense would give to the query. We must, however, keep distinctly before us and not confound the early and the advanced stage of the lung disease. I never hesitate in the early stage to relieve my patient of one source of trouble and suffering—of one drain on the resources of an enfeebled system, and cannot say I have ever seen untoward results from the arrest of the discharge. In the advanced phthisis the patient is not in a condition to be submitted to any surgical operation, however trifling, which can possibly be avoided, and we have further to encounter the unfortunate occurrence that the wound which we make may never heal, leaving a much larger surface for the secretion of purulent matter, and thus bring both operation and operator into disrepute.

Having determined on the *operation for fistula*, there are some matters which require attention, and I cannot too forcibly impress on you the important fact that on little points of detail, each in itself insignificant, the success of most surgical operations depends. These may escape the notice of lookers-on, but neglect of them will surely bring with it its own punishment. The surgeon should, if he wishes to be a successful operator, attend to them himself. On the day previous the bowel should be cleared by a dose of, say, 6 drachms of castor-oil with 2 drachms of tincture of rhubarb, and on the morning of the operation an enema of plain warm water should be administered. Cutting a fistula seldom demands the use of anæsthetics, unless in very nervous individuals, some of whom insist on having them. The external opening should be sought for. This may be close to the verge between two of the rugæ; it may be concealed by a sentinel papilla, such as we see leading to cloacæ over diseased bone; or it may be any distance out to the tuber ischii.

Velpeau's probe director being passed through the fistula until it enters the bowel, the forefinger, well oiled, should enter the rectum; by gentle manipulation the point should be hooked down until it projects through the anus. The instrument having a flat handle can be securely maintained with the groove in the proper position. A strong sharp-pointed knife should be rapidly passed along this, dividing the tissues bridged upon it. Should any difficulty exist in finding the internal opening, the search may be assisted by the finger in the rectum. The proposal to inject ink or milk for this purpose can seldom be necessary to anyone who has any use of his hands, and it only complicates and prolongs the operation. Now, you may ask me the question—Does it make any difference whether we introduce the finger or the probe first? I believe it does make a great deal of difference, both to the surgeon and to the patient. The finger, if introduced first, must press the walls of the fistula together, and by thus closing it obstruct the passage of the probe along its track.

The operation of Pott consists in the introduction of the finger, then the passage of a probe-pointed bistoury along the fistula, hooking it out through the anus, and cutting the tissues lying over the edge of the knife. This method is open to three objections—the edge of the knife may touch the fistula and cause the patient to start, the knife (if of ordinary construction) may break, the surgeon may cut his finger.

Should the inner opening be too far from the verge of the anus to admit of hooking down the probe, a lithotomy gouget may be slipped over the finger, so as to protect the opposite wall of the gut, and the division may then be effected along the groove of the director. If assistants are not to be had—as may often happen in country districts—a large tallow candle may be passed into the bowel, and the knife, having been pushed along the fistula, may be plunged into it, dividing the necessary tissues, without any risk of wounding the opposite wall.

In cases of "complex" fistula, or in those where the track runs very far from the bowel, it is not necessary to divide all the parts intervening between it and the mucous membrane. Such a proceeding would sometimes involve a dangerous gash in its wall, and might be followed by a very troublesome result—incontinence of fæces.

Should there appear to be no internal opening, and the probe is felt through the thin mucous membrane, it is good practice to push it through, making an internal opening, and then completing the operation. In cases of complex fistula it is not advisable to lay all the channels open by extensive incision. A free outlet may be made for treatment by injection and drainage. The operation for fistula may then be completed.

After operation the wound should be lightly dressed. A small piece of lint, well oiled, should be passed first into the bowel, and from that into the wound, so that it shall lie in it from top to bottom, through its entire extent. Now, it makes all the difference whether you pass the lint into the wound or into the bowel. In the former case you are likely to pass the pledget obliquely through the lower part of the cut into the rectum, leaving the upper part to heal by direct union, and thus lay the foundation for a return of the disease. The application of a bladder of ice or a cold sponge gives great relief from the smarting pain and diminishes the likelihood of hemorrhage. An instrument has been devised for irrigating the rectum with cold water, but the ice bag is more effectual.—*Dublin Journal of Medical Science, March* 1883, *p.* 273.

ORGANS OF URINE AND GENERATION.

52.—ON A NEW MODE OF RELIEVING RETENTION FROM PROSTATIC DISEASE.

By Sir Henry Thompson, Surgeon-Ext. to H.M. the King of the Belgians, Consulting Surgeon and Emeritus Professor of Clinical Surgery to University College Hospital.

I have long been anxious to discover a means of affording some permanent relief to those who suffer with severe and long standing prostatic disease. I refer to a condition in which the patient, having for several years relied entirely on the use of a catheter for the removal of all his urine, finds the bladder becoming so intolerant of its contents, that the act of catheterism, at first perhaps employed but three or four times in the twenty-four hours, must now be repeated under penalty of unendurable torture, at least every hour or hour and half. His time is indeed chiefly spent, both by day and by night, partly in suffering from retained urine, and partly from the painful catheterism for the sake of the temporary relief it affords.

It is more than fifteen years ago since I first attempted to mitigate this condition by tapping the bladder above the pubes and establishing an opening there by which to empty the bladder, and to supersede, for a considerable time at least, the otherwise inevitable frequent catheterism.

But the cases in which it appeared to be warrantable to adopt this somewhat grave remedy, were mostly examples of the disease in a very advanced stage; and, for three or four such, the last few weeks of a doomed life were rendered by that proceeding comparatively comfortable.

In two others, the disagreeable leakage of urine which took

place round the tube, greatly interfered with the patient's comfort, and with his ability to take exercise, &c. The plan seems therefore to be applicable only to the cases first referred to; and it appeared that the power to afford substantial relief to patients who, were it not for the severe local suffering, possess healthy constitutions and some vigour, was still a desideratum.

The essential element of the difficulty in these cases is evidently the presence of a severe chronic cystitis, from which there is no escape; since the continued mechanical interference, rendered necessary by retention, perpetuates and intensifies the cystitis. The patient is, in fact, the victim of a vicious circle of actions, in which an indispensable remedy, the catheter, aggravates the inflammation of the bladder, which therefore, in its turn, demands the instrument with increasing frequency.

It occurred to me, then, that were it possible to suspend all action on the part of the bladder for a few days only, to prevent any accumulation of urine with the organ, to allay the constant and painful want to pass urine, and also at the same time to abolish catheterism altogether, with its irritating effect on the urethra, the inflammation of the bladder might subside, and its tolerance of urine might considerably increase. And I hoped that a state of things might be subsequently brought about, similar to that which is present in a less aggravated stage of obstruction, when catheterism is not needed more than six or seven times in the twenty-four hours. If, in place of hourly relief by the instrument, an interval of three or four hours could be made to suffice, an enormous boon would be conferred on the patient.

This might probably be attained by a proceeding of the following kind. First, placing the patient in the lithotomy position, under ether, I proposed to pass a grooved median staff into the bladder, and make, from the raphé of the perinæum, a small vertical incision just above the anus, large enough only to admit the index finger—the incision to terminate in the staff at the membranous portion of the urethra, which should be divided for half an inch at most, so as to admit the finger to traverse the canal to the neck of the bladder. Then, having withdrawn the staff, I proposed to insert a large vulcanised catheter or tube, say about No. 20 (English scale), with its extremity just within the bladder, fastening it there by tape to a bandage round the waist; the tube to be retained as a channel for the urine, for several days at least.

An opportunity soon offered of making an opening in the manner described, for a patient at the age of sixty, who was passing the catheter every hour, and whose vital powers were

at the lowest ebb from constant suffering and loss of rest, but whose constitution was apparently sound; and I placed in the bladder, by the new passage, an India-rubber catheter, so that the urine might flow off continuously into a receptacle as fast as it arrived in the bladder. The relief was immediate, and most remarkable. He enjoyed long periods of unbroken sleep, and was unconscious of any pain; while the urine itself, which had been charged with muco-pus and blood, and had been alkaline and offensive in the highest degree, assumed in the course of a few hours a healthy colour, an acid reaction, and was almost clear. In two or three days, the patient had regained appetite and digestion, became cheerful, and showed a change for the better, which no one had been sanguine enough to anticipate. On the eighth day, I removed the catheter from the wound; during the next two days, urine issued by that route at intervals of some hours; but the wound, which was very small, rapidly closed, and the catheter was, of course, again necessary. But the passing of the instrument was no longer painful; the bladder was not inflamed, and could now retain urine three or four hours without inconvenience; while the patient himself, in less than three weeks from the operation, was enjoying an active life out of doors, having been long confined to his room in the suffering condition which has been described in general terms above. The operation was performed on the 20th of March last, in the presence of my friend, Dr. Chepmell, and others.

I saw my patient, a highly esteemed and well-known member of our own profession, on the 14th October last. The report, in his own words, was then as follows. "I use the catheter now only six times in the twenty-four hours, instead of eighteen or twenty times. The urine is a little cloudy—mostly acid, sometimes the reverse. I can drive for two or three hours in the afternoon without pain or fatigue, taking usually a morning walk of a mile or more. My general health is good. The degree of relief afforded by the operation can scarcely be exaggerated."

On the 30th of June last, I performed the same operation on a gentleman, eighty-three years of age, who was suffering from unusually painful and frequent micturition, the interval rarely amounting to three-quarters of an hour either by day or night. He could, however, nearly empty his bladder by his own efforts, and required the catheter only once a day; its employment affording very little relief. It was one of those rare examples of such a condition existing, in which no calculus and no organic changes in the urinary organs could be discovered. As his constitution was excellent, I did not hesitate (his age notwithstanding) to pursue the course described in the preceding case—

failing to find, on examination, either tumour of any kind, or calculus. The immediate relief, however, was so great, that I did not remove the India-rubber tube from the wound until the twelfth day; and he was then very unwilling to part with it, since he had not been so comfortable for upwards of a year. He had also been able to dispense altogether with the use of morphia, which, up to the time of the operation, he had been compelled to take in full doses. The wound rapidly healed; he retained urine from two to three hours, using his catheter only once in the day as before. Such was the report he gave me in the early autumn, as I left town. During my absence, he ventured to take much more exercise than he had been of late accustomed to, and had a relapse. When I saw him on my return (October 7th), he was gradually improving, and was very grateful for the change; saying he would gladly again submit to the operation, if anything like the old painful condition reappeared, as he had experienced nothing but relief from the proceeding. However, such a course does not at present appear to be necessary or imminent.

Such are two typical cases widely differing; the former an example of a comparatively common condition, the latter one of very rare occurrence. In both instances, I attribute the benefit to a temporary suspension of function in both the bladder and the urethra; in the bladder as a containing viscus, in the urethra as a channel or transmitting one. By means of the tube the urine leaves the body almost direct from the ureters, while the bladder and the urethra, being in a state of perfect quiescence, cease almost immediately to be inflamed; all muco-pus disappears, and the urine is discharged in as healthy condition as it leaves the kidneys—that is, free from any adventitious product of the passages. Another illustration of the extremely rapid recovery which the bladder is capable of making, even when it has been inflamed for a long period of time, which I have often had to note after removing a stone from its interior. Within six hours after emptying the bladder of broken calculous matter I have seen the urine, which had been for weeks charged with inflammatory products, flowing off absolutely clear. I cannot doubt, therefore, that this great recuperative power may be rendered available for the relief of obstinate cystitis produced by other causes than the presence of stone. By means of this simple incision, also, an opportunity is obtained of making "digital exploration of the bladder," on the plan which I recently proposed as a systematic procedure for the diagnosis and treatment of obscure cases. While the patient is fully under the influence of ether the operator is to make, with his right hand, firm suprapubic pressure, by which means he is able to bring every portion of the inner surface of the bladder

consecutively over the tip of the left index finger, introduced by the opening to the neck of the bladder, and thus to ascertain its condition and contents. I fancy that calculous deposit, or impacted calculus, is thus detected. Its removal may be accomplished, supposing it to be movable, as it mostly is. I have now performed the operation described in ten cases for various purposes, and in two of these I have been able to remove adhering and impacted calculus where its presence was little suspected, in one instance, with a remarkably successful result. Both these cases I had the advantage of seeing in consultation with Sir W. Jenner before deciding on the operation.

I am aware that proposals have long ago been made to "perform lithotomy" for cases of intractable chronic cystitis associated with enlarged prostate, no doubt partly on the ground that division of the prostate itself might be serviceable. This, however, is a measure of far greater risk than the operation I have employed, which is simply a very limited external urethrotomy, leaving both bladder and prostate free from any action of the knife. But I am satisfied that no benefit follows even the incision and dilatation which necessarily take place in the operation of lithotomy in prostatic cases, as far as restoration of the power of the bladder is concerned. I have performed the lateral operation for several patients who had been long previously unable to pass any urine except by catheter, and in operating I have sometimes removed considerable masses of tumour, and have always been disappointed subsequently at finding no improvement whatever in regard of the patient's power to micturate without artificial aid. The performance of lithotomy, then, ought not to be contemplated in any case with the object in view now under consideration. The withdrawal of the urine, however, by an opening into the membranous part of the urethra is a very safe and simple proceeding, and will, I am satisfied, afford more or less relief for a considerable period of time to patients whose sufferings are due to the action of very frequent catheterism upon passages which have long been severely inflamed.—*British Medical Journal*, *Dec.* 9, 1882, *p.* 1131.

53.—EXPLORATION OF BLADDER BY PERINEAL SECTION, FOR DIAGNOSING OBSCURE VESICAL DISEASES, AND REMOVING TUMOURS, ENCYSTED CALCULI, &c.

By Sir Henry Thompson, Bart., F.R.C.S., &c.

I have often discussed the ordinary affections of the urinary passages, and endeavoured to illustrate the best methods of dealing with them. I propose now to call attention to the unusual and obscure cases which sometimes occur, and to a new mode of investigating them, which is, I believe, destined to be

extremely valuable. It must be understood clearly at the outset, that the plan I propose is not to be resorted to with anything like frequency in urinary diseases, nor, indeed, is it to be thought of in any single instance until we have failed, after prolonged and intelligent inquiry, by the means generally adopted to ascertain the nature of these affections.

By way of indicating a typical example of the cases which are presented for careful investigation, because the affection is obscure, let me suppose a patient before us, who may belong to either sex, and who may be an adult of any age, of whom the following particulars are affirmed. He or she is the subject of unduly frequent micturition, both by day and night; the act itself is almost always painful, while pain, or at least uneasiness, more or less constant, is felt about the loins or pelvis. Then hematuria occurs with frequency, and is more considerable on some days than on others; but besides blood, the urine contains also pus and mucus. The signs and symptoms named have existed for a considerable period of time, and, although varying in intensity from week to week, have notably increased during the last few months. It is manifest also that the condition is not the mere sequel to an attack of acute cystitis, or to local poisoning as by gonorrhœa or the like; nor is it the result of any mechanical injury.

The method of inquiry best calculated to elicit ascertainable facts in such a case, consists in solving the following questions, and these are placed in the order in which it is generally best to determine them.

1. Has the urethra a sufficient calibre? To be ascertained by means of the bougie.

2. Does the patient empty the bladder by his natural efforts? To be ascertained by passing a catheter immediately after the patient's natural act of micturition.

3. Is any enlargement of the parts, either prostatic or vesical, notable on making rectal examination? seeking, of course, either the full, rounded contour of the hypertrophied prostate, or the hard irregular nodules of a cancerous growth.

4. Can the presence of foreign body in the bladder be demonstrated by the sound, or can any other abnormal condition be thus detected?

5. Are the signs of renal disease present in the form of constitutional albuminuria, habitual exfoliation of casts, of tubes, &c., or are there grounds for suspecting the presence of a calculus in the kidney, or possibly in the ureter, or of an abscess or tumour there?

Now, the whole of these queries may be answered in a sense favourable to the patient; in other words, no evidence of the presence of organic disease may be elicited by any one of these

inquiries, nothing, indeed, discovered beyond the simple facts which constitute the group of signs and symptoms complained of, and which are of themselves insufficient to determine the nature or the locality of the disease. Let me, however, remark here that observation of the urine from time to time sometimes reveals a sign of great importance not yet mentioned—viz., the presence in it of small fragments of tissue which, under the microscope, show structure resembling that of the fibromatous or villous growths which occasionally spring from the internal coat of the bladder. There is another sign of great importance, second only in that regard to the presence of débris. The stream of urine issuing as usual of the normal tint sometimes changes to a florid red before ceasing to flow. Such an occurrence ought to be verified as fact by more than one observation. When it does occur the bleeding is always vesical or prostatic; when the blood is not only florid but rather plentiful, tumour of some kind is almost always present. Such evidence is highly significant when associated with the history and symptoms just described.

The conditions thus sketched, however, are not those of a common case. In the great majority of our patients a moderately careful and intelligent examination places beyond doubt by adequate demonstration the nature of the disease which produces the symptom in question. Now, it is for the few exceptional cases, and these are always very important ones, with a history of hæmaturia manifestly not renal, already referred to, with a history of treatment, too, by internal remedies reputed to be styptic, and beyond this chiefly watched for the development of more advanced symptoms, but with no purpose of surgical interference—it is for these cases, I say, that I propose to take a new and decided action. And I desire very strongly to submit also that such action ought not to be postponed until a stage has arrived when the operation of diagnosing and efficiently treating the disease may probably be accompanied by serious danger to life. And on this ground especially, that if the cause of the hæmaturia be tumour or growth of any kind the issue will certainly be fatal, unless the disease can be removed by operation.

The first and the essential step in the method I propose consists in *examining the entire internal surface of the bladder with the finger*, by which means we can easily recognise the presence of any tumour, large or small, or even of an irregularity or roughness in the surface of the lining membrane, or the existence of any calculus lodged in a sac or sinus, or otherwise hidden from the researches of an ordinary sound passed by the usual route through the urethra. Of course it must be sufficiently obvious to everyone that the ability to do this easily,

effectively, and safely, offers a valuable addition to the means ordinarily employed, for the most difficult and hitherto intractable cases of urinary disease which come before us.

But you naturally ask me, Is it possible that such an examination can be effected on the terms just named, that is, with ease, efficiency, and safety? I have no hesitation in answering you in the affirmative. I am, however, quite free to confess that a few years ago I should have been unable to make such a reply, and that it has only been after some unusual experiences which have fallen to my lot during the last few years, that I have satisfied myself that we can, without difficulty, examine the entire surface of the bladder with the tip of the finger.

Certainly before the discovery of anæsthesia the proceeding would have been impossible. But mere anæsthesia is insufficient: it is essential, in the first place, that the influence of the anæsthetic agent should be carried far enough to ensure complete relaxation of the voluntary muscles, so that their tension should not impede any action of the operator. This condition being fulfilled, it is, in the second place, necessary that the tip of the index-finger should be placed just within the internal meatus at the neck of the bladder. Next, it is to be remembered that when the bladder is empty—that is, not distended by urine or by a calculus—the cavity presented for exploration is a small flaccid bag, sometimes merely a cul-de-sac, every portion of which is situate at a short distance from the finger placed in the position just described. Lastly, if the finger of the operator's left hand be maintained in that position, while he makes firm suprapubic pressure with his right hand, I maintain that there is no difficulty in bringing every portion of the internal surface of the bladder, successively, into close contact with the tip of the left index-finger; a contact which enables him at once to estimate correctly the condition of the interior, in relation to every important particular necessary to be ascertained.

Before considering the best mode of performing the simple operation which enables this exploration to be effected, I will anticipate an objection which may very naturally arise in some minds—viz., that we are not always able to reach the neck of the bladder with the tip of the finger when the incisions for removing a stone by the lateral operation have been completed. Hence it is notorious that a long and flexible index-finger is regarded as an important element in the making of an able lithotomist; yet, although possessed of it, he may still be unable to reach the point in question, in a few exceptional cases.

To this I reply, that there are few bladders indeed into the neck of which, even when the prostate is large, a finger of

ordinary length may not be introduced, if carried straight in from the centre of the perineum, which is the shortest route from the surface. But, in addition to this, I refer to the fact which I have verified by experience—viz., that firm suprapubic pressure made by the right hand of the operator, or aided by an assistant if necessary, will accomplish the object, even in an exceptionally deep perineum, if the index-finger is firmly pressed up from the perineum to meet the other hand. Of course, when large outgrowth of the prostate is present, occupying the neck and cavity of the bladder, it may be impossible to carry the finger to the point desired; but then it is to be remembered that in such circumstances there is no need to make incisions in order to explore the internal surface of the bladder, the case not being an obscure one, but, on the contrary, quite unmistakable.

Let us now consider the manner of operating, so as to place the tip of the left index within the neck of the bladder. The first object to be attained is the opening of the urethra at or about the membranous portion, by the simplest and shortest route from the surface of the perineum, making a passage only sufficing to admit with ease the entry of the forefinger and no more. There can be no doubt that a vertical median incision—that is, one made in the line of the raphe—will fulfil this indication better than any other. In this situation the prominence which is formed by the bended knuckles of the operator, when introducing as far as possible the index-finger, lies in the hollow equidistant between the two nates, and presses directly towards the centre of the bladder. Any incision made right or left of the median line must of necessity lead obliquely to the centre, and be therefore a longer line, because it commences at a point on the external surface more distant from the neck of the bladder than is the raphe of the perineum. Accordingly I prefer, and always employ, a vertical incision in the centre of the perineum, carried straight to the urethra, aiming at the point close bebind the bulb, a procedure which as far as the incisions are concerned has been practised by surgeons for centuries to relieve stricture, retention, &c., objects, however, altogether differing from that for which I have employed it, and which is the subject of our consideration here.

The patient then is to be placed in the ordinary position for lithotomy, a rather short, well-curved staff, with deep median groove is passed into the bladder. The operator enters the point of a long, straight bistoury about three-quarters of an inch above the anus, and makes a vertical incision upwards of the skin, not more than an inch or an inch and a quarter in length. He then carries inward the knife deeply, nearly parallel with the rectum, in which the left index should be

placed, to inform the operator of the relative positions of the blade and the bowel, until the point reaches the groove of the staff about the membranous portion of the urethra. He is next to incise the tissues covering the groove for about half an inch; the left index is then to be introduced into the wound, the nail insinuated into the groove, then slowly into the urethra beyond, gradually dilating it. When the finger is fairly lodged in the prostatic urethra, the staff is withdrawn, and the finger is pressed firmly onwards until it enters the neck of the bladder; and it should now be felt to fill, or nearly so, the entire wound, and it

FIG. 1.

The forceps for removing tumours (No. 1).

FIG. 2.

Curved forceps (No. 2), for tumours springing from the side of the bladder.

does so if the incisions have been made as directed. As a rule very little bleeding results. The operator now, maintaining his finger in the situation described, should rise from his seat to the standing position, place his right hand on the patient's abdomen, directly above the pubic symphysis, and make firm pressure into the pelvic cavity. He will soon recognise the end of the left index-finger, and by concerted movements of the two hands he may, as I have already said, examine with ease the upper surface of the bladder, and explore the lateral walls,

the fundus, and trigone; the latter more completely, if necessary, by placing the other index-finger in the rectum.

You will observe that the procedure thus described is simply a limited external urethrotomy, not "cystotomy," as when incision of the neck of the bladder has also been made; much less is it "lithotomy" in any form. The incision involves only a small portion of the urethra anterior to the prostate and neck of the bladder, which are left perfect and intact throughout. Hence its result is to enable the male bladder to be examined almost as readily as that of the female in its natural condition, an approximation to which is attained by the operation; and in the female, as you know, dilatation of the urethra alone suffices to permit the finger to enter the viscus and explore it.

We will now suppose the result of such an exploration to have been that the operator's finger recognises the presence of a soft protruding mass or flocculent growth springing from some part of the bladder. If brought within reach of the finger by supra-pubic pressure, it is easy to verify the nature of the pedicle, whether it be narrow or broad, together with any other physical characters which are obvious to the touch. The operator will next introduce a forceps, the extremities of which meet by broad roughened edges, so that they nip off, without cutting, the greater part of the salient portions of the tumour. Those that I have designed for the purpose are shown at Fig. 1. The extremity of each blade meets its fellow by a margin of about an inch long and about one-sixteenth of an inch broad, and these are indented so as to effect a separation of the morbid tissue, with as little chance of producing hemorrhage as possible. With this instrument the greater portion of almost all growths can be removed. But I soon met with a case in which a small growth protruded close to the neck of the bladder, and although with the forceps described I took away the protruding portions from the other aspects of the bladder, I failed with this instrument to seize the small one close to the neck. For this I designed another forceps (No. 2, Fig. 2). It will be readily seen that by means of this instrument it is easy to remove any growth in the situation described, and both forms should be at hand when making exploration of the bladder. Since the occurrence referred to I have operated in three other cases of large vesical tumour in the male, springing from the left side of the bladder not far from the neck. These I removed chiefly with the curved forceps (No. 2), the other forceps being almost inapplicable (see Figs. 2, 3,). Perhaps there are circumstances in which an écraseur may be a more efficient instrument than the forceps for removing a growth; this, however, I venture to doubt. In order to adjust the cord or wire, more room is necessary for manipulation than the incisions described will furnish. It is better to keep these

within narrow limits when it is possible, and such have sufficed me for the complete removal by the forceps of a very large tumour.

FIG. 3.

The same forceps, or No. 2. Front view, showing divergence of blades by joint, to prevent nipping of the soft parts at neck of the bladder when closed.

The forceps and the finger are of course to be employed alternately, the one to define clearly what the other is to lay hold of; for I have never found it necessary to make a wound large enough for both finger and forceps to pass in company. But the écraseur would require an accompanying finger, and more space must accordingly be secured for their joint action, which I object to because it is certainly undesirable to make extensive incisions. In the female the écraseur can be more easily managed, because room is more readily acquired without section.

After the chief portions of tumour have been removed from the bladder, it should be washed out with a current of cool water, by means of a syringe and tube through the wound, after which a large vulcanised catheter is to be introduced, so that about half an inch lies within the bladder, and in this position tied by stout bobbin to a bandage round the patient's waist. The other end protrudes some five or six inches, and is placed within a bottle, adjusted to receive the contents of the bladder. Hemorrhage is usually free during twenty-four to forty-eight hours, and then slowly ceases; pain is sometimes but not always severe, and should be controlled when necessary by a morphia injection. The tube should remain from five to eight days and then be removed, when the wound rapidly heals and all the urine soon passes by the natural channel.—*Lancet, Feb.* 3, 1883, *p.* 181.

54.—ON A CASE OF LITHOTOMY, AND ENUCLEATION OF A TUMOUR OF THE PROSTATE.

By REGINALD HARRISON, F.R.C.S., Surgeon to the Liverpool Royal Infirmary.

I desire to record this case, not as presenting any novelty in treatment or in operative procedure, but as bearing upon the remedying of prostatic enlargement by means other than those commonly recognised.

If the patient, whose case I am about to relate, had not had a stone in his bladder, he would not in all probability have been cured of his large prostate, or if his stone had been of a size or of a nature which would have permitted of its being removed by any other than a cutting operation, though he might have been relieved of it, he would have been none the more rid of those other urinary troubles which usually attend a large prostate. To the two-fold accident of having a stone in his bladder, and of having it of a size and constitution which did not admit of lithotrity, this patient owes a completeness in recovery which could not otherwise have been attained. Cases such as these, of which mine is not a solitary example, are instructive as suggesting whether it is not possible to apply that portion of their success which is the result of accident, rather than of design, to similar though uncomplicated conditions, where palliative measures, directed towards relieving certain consequences of prostatic hypertrophy, are found to be insufficient. It was remarked by the late Sir William Fergusson, in a communication on this subject to which I shall again refer, "When stone is combined with enlarged prostate who can say from which cause the distress is greatest?" Cases are not infrequent where the symptoms produced by a large prostate are even more urgent than those attending a stone in the bladder. The following are the particulars of the case:—

W. B. H., a Custom-House officer, æt. 67, was admitted into the Liverpool Royal Infirmary under my care, on September 2nd, 1881, having been referred to me by Dr. Samuels. He had been suffering from symptoms of stone for seven years. I sounded him and made out that his bladder contained a large oxalate of lime calculus; further, that his prostate was enlarged. On Sept. 5th I performed lateral lithotomy; on seizing the stone I found it was so large that I made a bilateral section of the prostate, a course I was prepared for, and which I have successfully adopted on two previous occasions where I had to remove hard stones, weighing over two ounces. Though this extension of my deep incision gave me additional room for extraction, I recognised that the enlarged prostate still remained an obstacle. As a portion of the prostate seemed loose and disposed to come away, I enucleated it with my forefinger, and slipped it out, when I was able to remove the stone from the bladder without force. There was no bleeding worth mentioning either at the time of or after the operation. The patient made a good recovery and left the Infirmary on November 5th. There was nothing to remark about the temperature during the whole of the treatment. The tumour removed was about the size of a walnut, and it will be seen that it was almost divided into two portions by the incision into the prostate. In struc-

ture it may be described as an adenoma, analogous to what is commonly observed in the breast, and corresponding with the description given of these growths by Sir W. Fergusson. The stone was an unusually large specimen of oxalate of lime, covered with small spines, some of which were broken off by the forceps during extraction. It weighs two ounces and five drachms.

By the kindness of my colleague, Mr. Bickersteth, I am enabled to give the particulars of an unpublished case very similar in many respects to that which I have related. A country gentleman, aged sixty-three, had suffered from symptoms of stone for fifteen years. Mr. Bickersteth, considering that it was a case for lithotomy, performed the lateral operation. Finding as he had anticipated that the stone was large, he extended the deep incision and made a bi-lateral section of the prostate; this enabled him to make out that in addition to the calculus he had a large growth connected with the prostate to deal with. After extracting the stone with the forceps he shelled out with his index finger a mass about the size of a hen's egg, which proved on examination to be an adenoma of the prostate, not a true hypertrophic growth; the stone weighed nearly two ounces and a half. The patient made a good recovery, and to the present date remains perfectly well. The operation was performed on October 21st, 1878.

In 1870 the late Sir W. Fergusson drew attention to the subject of lithotomy in connection with enlarged prostate, and narrated a case where, in a patient eighty years of age, after removing the stone by lithotomy he extracted the lower part of the prostate with the finger as readily as if it had been a stone. The patient not only recovered from the operation but never showed any further signs of prostatic irritation. The paper to which I have referred concludes with the remark: "I have thus ventured to put on record what some of my professional brethren may have hesitated to do from a fear that they may have been guilty in their operations of perpetrating some rough mechanism not in accordance with that nicety of manipulation which is thought so essential in the performance of the master handiwork in surgery—lithotomy."

Mr. Cadge has reported a case where he removed during a lithotomy, in the forceps between the joint of the blades, three masses which were found to be fibrous outgrowths of the prostate. In commenting upon this case, Mr. Cadge says: "In about two months the wound was perfectly healed; there is incontinence, which may be in a great part due to the removal of the prostatic tumour, but it must be remembered that it also existed before the operation. It has happened to me twice before to remove small fibrous tumours of the prostate gland

during the operation of lithotomy and apparently without harm to the patient." More recently Dr. C. Williams, of Norwich, has reported a case where he removed, accidentally, between the forceps an enlarged middle lobe of the prostate. In three weeks the patient was reported as recovered, having seldom to micturate more than once in the night.

These illustrations show that two kinds of growth have been removed from the prostate gland in the course of lithotomies, namely, (1) isolated tumours, resembling adenomata, the term used by Sir W. Fergusson, and (2) ordinary outgrowths or hypertrophies, such as we are most familiar with, as affecting the middle lobe. From the illustrations I have given I submit we may draw three conclusions at the least.

First, that lateral cystotomy may be practised in certain cases of enlarged prostate which are attended with symptoms producing great distress with the view of exploring and if possible of removing the growth.

Second, that in all cases of cystotomy for calculus where the prostate is found to be enlarged, that a careful search should be made with the finger, with the view of effecting the removal of the growth should such be found practicable.

Third, that in determining the selection of lithotomy or lithotrity in a case where stone in the bladder is complicated with enlargement of the prostate, regard should be had to the possibility of removing both of these causes of distress by the one operation, namely, by lithotomy.

Further, the cases I have recorded seem to indicate the mode in which these growths may best be removed when met with, either in the course of a lithotomy, or a cystotomy performed for the purpose. The presence of isolated growths in the prostate can be ascertained when the gland is opened into, by exploration with the finger; for, as Sir W. Fergusson observed, "as the finger passes towards the bladder, the sensation is as if its point glided through several rounded bodies in the substance of the gland, which are but slenderly in contact with each other." Of this sensation I have been conscious in more than one lithotomy I have performed in elderly persons. Thus discovered these growths may then be enucleated by the finger as I have already endeavoured to demonstrate.

When the growths assume a more pendulous form, as we frequently see when the third lobe is hypertrophied, though they may be detached with the finger or the lithotomy forceps, as occurred in Dr. Williams' case, I think a more precise and possibly safer proceeding might be adopted. I refer, after the bladder has been opened and the growth explored with the finger, to the including of the growth in some simple form of écraseur by which it could be clearly detached without risk of

hemorrhage, just as is sometimes done in the case of a uterine polypus. In referring to such a proceeding, it is with the view of making preparation for what may be necessary when undertaking the operation of lithotomy in a person who is known, or suspected, to have a large prostate, the obstructing portion of which it may be desirable to remove. In none of the cases I have brought forward was the removal of the growth complicated or rendered dangerous by hemorrhage either at the time of operation or subsequently. — *Medico-Chirurgical Transactions, vol.* 65, 1882, *p.* 39.

55.—ON A SIMPLIFIED EVACUATOR FOR LITHOLAPAXY.

By Prof. H. J. Bigelow, Harvard University, U.S.

The operation for the immediate removal of a calculus through a catheter, like many other surgical operations, can be accomplished more or less satisfactorily by any one of several instruments which much resemble each other. But it can be done better by employing a more perfect apparatus than those now generally in use. It has been said that "no new form of instrument is required by this operation," which is true so far as it implies that neither a lithotrite nor an evacuator is a new instrument. But it would be a mistake to infer that the operation could have been done with the instruments of the old lithotrity, and that they had needed no change to adapt them to what is now required of them, or that they cannot be still further modified to advantage. The new operation cannot be performed with the old instruments. It requires a larger evacuating catheter than that of Clover, through which the usual product of the lithotrite could not pass, except powder and sand, and that only in limited quantities, because the other detritus obstructed the entrance of the tube. Though at first received with a good deal of distrust, the large catheter has been finally adopted by all the surgeons who have done the operation, and in fact cannot be dispensed with. It should be combined with a thoroughly efficient aspirator; but no particular form of aspirator has so far met with general approval. Though better than it was, surgeons have none as yet which entirely satisfies all requirements of the operation, and is at the same time compact and convenient to handle, and simple in construction. This part of the evacuator still needs improvement. The usual parts of an evacuator, not including the catheter, are these:—1. The exhaust, the best form of which is an elastic bulb. 2. A space or trap for air at the upper part of the instrument. 3. A glass receiver at the lower part, to collect and show the débris.

In drawing out fragments from the bladder through the large

catheter, one bulb or aspirator, if strong enough, is about as efficient as another. An aspirator of almost any shape, and having almost any combination of its parts, will do this. So will a mere elastic bulb attached directly to the catheter, without joints or receiver, if it is placed lower than the catheter, bent down like the body of a retort, so that the fragments can fall to the bottom of it; and the instrument will still work well if it has joints made, for economy, of cork or rubber instead of metal. But, however otherwise arranged, a satisfactory aspirator should have—4. Some device, near the catheter, to act as a trap for débris and secure every fragment that has passed it.

The chief difference among evacuators now is in the certainty with which they retain the fragments they have aspirated. Any instrument will draw out the fragments, but few hold them securely, for the débris do not always fall into the glass receiver, nor do they always remain in it. On the contrary, they are easily carried back to the bladder. This defect in the action of the evacuator has received little attention from surgeons, although it is the only point connected with the instrument which offers any difficulty whatever. Until recently it has been remedied only by sacrificing simplicity in the apparatus.

In endeavouring to make a satisfactory evacuator for lithotapaxy, many experiments have to be tried. It is quite possible that a perfectly satisfactory instrument might have been contrived some time ago if it had been generally understood that an evacuator that works best with pieces of broken coal in a vessel of water will succeed best with the fragments in the bladder. So also will the surgeon if he is otherwise well qualified. It is true that the living tissues are easily injured, but in other respects the experiment can be made sufficiently like the operation to give it great value. Aspirating débris from the bladder is not a question of pathology, but of operative surgery; of physics. And in view of the fact that we fail in some bladders to discover a last fragment even by repeated washing, an evacuator should be so constructed that it will absolutely prevent a fragment that has once passed the catheter from returning to the bladder to become the nucleus of another calculus.

It is not altogether easy to meet this requirement, because the solid particles are usually borne back and forth with the current of water. In a common evacuator, they are carried wherever it goes, first from the bladder to the bulb, and then, when it is reversed, back to the bladder, a part only falling into the receiver at each aspiration. As we may fairly assume that a surgeon would not deliberately inject foreign bodies into a patient's bladder, there must be something wrong in a system which obliges him to do this, and makes it necessary to aspirate

the same débris twenty times over in order to remove it. In short the apparatus as commonly arranged is still a defective one, and needs some special contrivance to assist the action of gravity in securing the débris.

Surgeons have long felt this. The use of a long elastic tube connected with the catheter has been more than once criticised, and with some reason, on the ground that it might contain fragments which would be returned to the bladder. And again, in order to shorten by an inch the route from the bladder, a less convenient stopcock has been substituted for the usual one. But lithotritists should be fully aware of the fact that whether there is an elastic tube or not, a tenfold greater quantity of fragments is generally driven back out of the bulb itself, and that the difficulty lies almost wholly in that part of the instrument. At each expansion débris are drawn from the bladder into the bulb, where they are detained until, when it is compressed, they are injected back into the bladder. Only a part of them, sometimes only the larger half, the quantity varying in different instruments, settle into the glass receiver. This important fact, so little recognised, should not be accepted without demonstration.

An instrument which Sir Henry Thompson has lately abandoned can be made to demonstrate exactly how the currents act upon the fragments in an evacuator which is unprovided with a catheter-trap to prevent them from re-entering the catheter. It is here selected because the peculiar form of this instrument makes it easy to fit a glass tube to it, so that we can see what takes place in the interior. Let a piece of glass tube, an inch in diameter, be inserted at the joint between the bulb and the catheter, to show what passes with the current from one to the other in either direction. If the end of the catheter be now placed in a suitable vessel of water containing fragments of coal of different sizes, while the bulb is alternately compressed and allowed to dilate, a continued stream of fragments will be seen rising from the vessel into the bulb, and then returning to the vessel, as they inevitably do to the bladder. The back-flow of débris can be still better watched if a glass tube be also substituted for the catheter.

But there is another important fact illustrated by this instrument. Fragments do not always stay in a receiver after they have been deposited there. When the glass receiver of this evacuator is half filled with fragments, a part of these are easily carried back into the bladder or into the vessel. They are first lifted up from the receiver into the bulb, and then driven out through the catheter. For though the orifice of this glass receiver is small and protected by a special trap, the current and débris pass out of it as well as into it. It could not

have been foreseen that fragments would escape from a receptacle apparently so well arranged. But it will be found that in any instrument, if the bulb or catheter directs the current into the glass receiver, whether directly or obliquely, fragments are easily carried out again. And the general result is little better if, to avoid stirring the fragments which lie in the receiver, the current is directed horizontally over the mouth of it instead of into it. Some of them then pass directly back and forth between the bladder and the bulb, over the receiver without falling into it. This defect can be shown in an instrument recently employed by Sir Henry at the suggestion of Weiss & Co., where the stream from the catheter passes horizontally through an empty chamber on its way to the bulb. As the straam enters it, its velocity is so diminished that fragments fall to the bottom into the receiver in greater number when the bulb is weak. Many fragments as usual are secured. But in order to be wholly effectual, the chamber intended to retard and break up the current by its size would have to be inconveniently large to give time to the floating débris entering on one side to settle into the receiver without passing further. The principle here involved is quite different from that of the evacuator above described. This instrument is not unlike one figured in the Lancet, but the valve and strainer which there act as a trap have been omitted, and, in consequence, not a few fragments escape back to the bladder.

In Weiss's evacuator again, some of the fragments which enter the bulb gather in the bottom of it, which is lower than its outlet, and where there is no receiver to collect them. The chief difficulty, however, is not that these fragments stray into the bulb, but that for want of a trap they are afterwards liable to escape out of it to the bladder. This difficulty is not wholly obviated by placing a strainer across the mouth of the bulb to prevent the fragments from entering it, as has been done in some other evacuators.—*Lancet, Jan.* 6, 1883, *p.* 6.

56.—SIR HENRY THOMPSON ON BIGELOW'S EVACUATOR.

[Sir H. Thompson replies as follows to Prof. Bigelow's criticisms in the foregoing article, in which, he says, "statements are made relative to my instrument which are extremely inaccurate."]

First, Prof. Bigelow describes and draws an aspirator of mine, which he says I have "lately abandoned"! So far from having done so, I rarely operate without it, and use it nearly as often as the more recent model.

Next, the drawing of this aspirator is intended to prove that its action is so defective as to return into the bladder a large portion of the débris already removed. I have, moreover, long

known that Prof. Bigelow has been in the habit of exhibiting one of my aspirators for the purpose of publicly illustrating its alleged defects (with bits of coal and water). I do not make any great complaint of this, although it is a mode of controversy to which just exception might be taken.

All I have to say is this, that nothing is easier than to use another man's instrument, in that man's absence, so as to make the instrument appear inefficient. *But it is not so in my hands.* Had Prof. Bigelow ever seen me use it—as scores of his compatriots have—he would know that, if properly used, there is no reflux of débris into the bladder. The best instrument in the surgical armamentarium may be misused, and grossly too, and such is the fate of mine in the professor's hands if he meets with the result which he is at such pains to publish to the world.

I have performed the operation of lithotrity at one sitting more frequently, probably, than any living operator; and I am delighted with it. Will anyone say—and there are abundant witnesses—that it is not a rapid and complete proceeding in my hands? How could that be possible if my aspirator is so defective?

And now I have only to congratulate Prof. Bigelow on his present search for "simplicity." He well knows that I regard his suggestion to remove the stone at one sitting as a great advance. But his first instruments were a return to the time of Heurteloup! After years of patient experience, the mechanism of both lithotrite and evacuator had become marvellously simple and efficient. This simplicity he disturbed, disastrously, for a time; exhibiting elaborate and costly apparatus at the Congress here in 1881, wholly useless to the practical lithotritist. He could not see that not one new instrument was required to carry out his excellent idea. As we were to attack larger stones, we wanted larger and stronger instruments—that was all. Clover's bottle, a little modified perhaps (as I suggested last January in the Lancet) is as good as, if not better than, any, and I still often use it. Only make it larger than before, and attach a larger evacuating catheter, *when you want a larger*, not otherwise. All the perforated tubes and strainers get so blocked with débris (as I found long since) in the human bladder—not with coal in water—as to be practically useless there.—*Lancet.*

57.—ON CALCULUS IN THE FEMALE BLADDER.

By Christopher Heath, F.R.C.S., Holme Professor of Clinical Surgery in University College, London.

The patient is a married woman, and had been confined six weeks when she presented herself in the out-patient department, complaining of pain about the bladder and frequent micturi-

tion. I had no difficulty in detecting a large mass of stone in the bladder. When placed upon the table in the lithotomy position, I proceeded to dilate the urethra with a pair of polypus forceps, and then introduced my little finger and subsequently my forefinger without difficulty. In this rapid dilatation there is of course some laceration, and it is usually upwards under the pubes, and does no harm. My finger enabled me to feel a very irregular mass of phosphatic stone, which I proceeded to break down with small lithotomy forceps, and I was able with these, with a scoop, and by washing out through a vulcanite speculum, to remove the débris which we have in the bottle. After clearing out all the fragments, I found that the bladder was in part encrusted with phosphates, and this mortar-like material I scraped away with the nail of the forefinger and a lithotomy scoop, until I was satisfied, by compressing the bladder with the other hand so as to sweep the finger over the whole interior of the viscus, that the mucous membrane was clear. Then, lastly, through the little vulcanite speculum, I applied a solution of nitrate of silver (a drachm to the ounce) over the whole surface of the interior of the bladder, and sent the patient to bed with a half grain of morphia suppository in the rectum. I expected that there would have been more or less complete incontinence for a day or two, after which the patient would have recovered perfect control; but this was not the case. She was able to hold her water completely on the day of the operation, and passed from the first acid urine, instead of the abominably offensive ammoniacal urine which had been present before. Now, this I find the invariable result of mopping out the bladder with a strong solution of nitrate of silver; and some of you may remember a man in whom the same thing was seen after lithotomy. The woman is now taking her food well, and is fairly convalescent, so much so that I have allowed her baby to be put to her breast again.

The symptoms of stone in the female closely resemble those in the male, except that, from the close propinquity of the bladder to the uterus they may be referred to the latter organ. Frequent micturition, pain especially after emptying the bladder, with "bearing down," should direct attention to the bladder and the condition of urine, which last is probably thick, and may occasionally contain blood. The detection of a stone with the sound is comparatively easy, for the urethra is short, and there is no prostate behind which a small stone can lie hid, whilst the ease with which the fundus of the bladder can be simultaneously examined with the finger in the vagina makes the diagnosis simple enough.

Calculus in the female may be of renal or vesical origin, or both, just as in the male; but there is the peculiarity about

stone in the female that it may occasionally have for its nucleus some foreign body introduced by the patient herself. If the foreign body is small, such as a piece of cork, and is completely enveloped by the calculous matter, it in no way complicates the treatment; but cases are not very uncommon in which a hair-pin or some other implement has been introduced into the bladder, and having escaped the patient's grasp, has been left there to form the nucleus of a concretion which is certain to form. In these cases the foreign body projects from the stone, and may not improbably have pierced the bladder, and given rise to serious complications.

Stone is not nearly so common in the female as in the male, the proportion between the two sexes being, according to Mr. Poland, one in the female to twenty or twenty-three in the male; and the same author remarks that "statistics respecting stone in the female, the operation and its consequences, and the mortality after operation, are incomplete and unsatisfactory." I am inclined to think, from what I have seen, that stone is more common in the female than is here stated, or than is generally supposed, because in all the cases I have had under my care the disease had not been recognised until shortly before I saw them, although the symptoms had long been present, and had been for the most part referred to uterine disorders. Irritability of the bladder is so common an accompaniment of uterine disease that it is easy to understand how both patient and doctor may be deceived by the symptoms produced by stone; but I would warn you, in all cases of uterine complaint, not to be satisfied without making both a rectal and a vesical examination if the symptoms are at all obscure or do not yield readily to appropriate treatment. When a vaginal examination is being made, it is so easy to pass the finger into the rectum and to slip the uterine sound (if no more convenient instrument is at hand) into the bladder, that there really is little excuse for overlooking disorders of any of the pelvic viscera.

The treatment of stone in the bladder of the female has been simplified almost as much as that of calculus in the male by the "one-sitting" method of lithotrity introduced by Bigelow. In former years I should have hesitated to break up a stone weighing an ounce and have probably had recourse to vaginal lithotomy, of which I shall speak presently. Of course lithotrity for small stones has been often performed in the female, and I ventured some years ago thus to break up a small mulberry calculus, the fragments of which may be seen to be covered with phosphates. The ordinary lithotrite, or a shorter one made for the purpose, may be readily employed, or where, as in the great majority of cases, the stone is soft and friable,

recourse may be had to simple lithotomy forceps and the crushing power of the hands. Bigelow's, Thompson's, or Clover's evacuator may be employed in the female as in the male, but I think I saved time in my case by using simply a vulcanite urethral speculum and washing out the fragments with an ordinary Higginson's syringe.

Extraction of small stones per urethram with polypus-forceps or a scoop is readily performed if the urethra is rapidly dilated in the way I have described; but it is never worth while to drag out large calculi at the risk of producing incontinence, when they can be so easily reduced in size by crushing. The method is, however, very satisfactory for the removal of the foreign bodies which occasionally find their way into the female bladder. Various ingenious instruments have been contrived for catching, doubling up, and extracting such articles as hairpins; but I have found practically that with the finger introduced through the urethra there is no difficulty in guiding a pair of polypus-forceps, and extracting a hairpin or other similar body without the risk inseparable from a supra-pubic lithotomy, which has been performed in similar cases.

The operation of lithotomy in the female resolves itself into a very simple proceeding—viz., cutting through the vaginal and vesical walls where they are in contact, and is hence called vaginal lithotomy. Let me remind you that the peritoneum does not touch the anterior wall of the vagina, which may therefore be divided quite up to the os uteri without risk to that membrane, whereas the pouch of Douglas lies between the upper part of the posterior vaginal wall and the rectum, and is liable to injury by obstetric instruments, &c. Vaginal lithotomy is no new operation, for it has been long known to surgeons, but the great modern improvement in it is the practice of closing the wound immediately by wire sutures so as to obviate the formation of a vesico-vaginal fistula. I show you three large stones which I removed from different women by this proceeding some years back. The first stone weighs one ounce and a half, and is remarkable, as you may see in the section, for having consisted originally of three angular calculi, which had become fused together in a mass of carbonate and phosphate of lime, measuring 2 inches by $1\frac{1}{2}$, and being 1 inch thick. This I extracted from a woman aged forty-nine, in whose bladder I detected a very distinct pouch, in which no doubt the stone had originally lain. I closed the wound with tin wire sutures introduced through the whole thickness of the bladder and vagina, and the patient made a perfect recovery. The second stone is nearly circular in shape, its long diameter being $2\frac{7}{8}$ in. and its short diameter $2\frac{3}{8}$ in., and its greatest thickness $1\frac{1}{4}$ in. It weighed two ounces, and I extracted it from a woman aged

forty who had long suffered from pain supposed to be uterine. In this case the closure of the wound was not entirely successful, and she had slight incontinence when in the upright position, but declined further interference. The third stone consists of one large mass and a quantity of débris, weighing altogether three ounces and a half, which I extracted from a woman aged fifty, who made a perfect recovery.

You will thus see that there is no difficulty in extracting very large calculi from the female bladder entire, but it is in some sense a reproach to our art that stones should be allowed to attain such a size. Early detection is as desirable in the female as in the male, and the great majority of such cases can be best treated by lithotrity at one sitting.—*Lancet, Dec.* 23, 1882, *p.* 1067.

58.—IMPERMEABLE STRICTURE.—PERINEAL SECTION.

By Timothy Holmes, Surgeon to St. George's Hospital; Surgeon-in-Chief to the Metropolitan Police Force.

We have had occasion lately to witness two operations for stricture which illustrate unusually well the classes of cases in which the operation of dividing the strictured portion of the urethra with the knife is indicated. Such an operation is rarely necessary, for there are few cases of stricture in which an instrument cannot be passed; and almost all cases which admit the passage of an instrument are susceptible of gradual dilatation. There are, however, a few cases in which, though the instrument can be passed, yet the stricture cannot be dilated, and it is in the treatment of these latter that the ingenuity of surgeons is chiefly exercised.

I propose to speak first of the cases in which no instrument can be passed, and then of those in which, though the stricture is not absolutely impassible, no progress can be made. The former is, as I said, a small number, and some surgeons, like the late Mr. Syme, would almost question its existence, believing that with dexterity and patience all chronic obstructions will at some time or other permit of the passage of some instrument. The acute cicatrisation which in some cases follows on rupture of the urethra, and which has been known to proceed so far as to entirely obliterate its tube, is of course a different affection. I am disposed to admit this teaching to a very great extent, and to allow that almost every stricture will become passable after prolonged rest in an equable temperature, free purging, assiduous warm bathing, the free action of the skin, avoidance of drink and unwholesome food, and abstinence from sexual intercourse. And in support of this view I notice that Sir H. Thompson, in his recently published "Clinical Lectures on

Diseases of the Urinary Organs," says he has only had occasion to perform perineal section for impermeable stricture three times in his life, and that in two of these cases the stricture was traumatic. We have a much more extended experience of perineal section at this hospital—whether from accidental causes, or from lack of the necessary dexterity, I will not presume to say. Certain it is that the old operation of perineal section—meaning by that term the operation of external urethrotomy without a guide passed through the stricture—has been comparatively often performed here, both by myself and my senior and junior colleagues, and on the whole, I think, with very good results.

The case on which you saw me operate a short time ago seems to me to illustrate unusually well those in which this operation is indicated, and the manner of performing it. The patient, A. O., a man over forty years of age, has led a wandering and dissipated life in India and Africa, has suffered from many of the disorders of health incident to tropical climates, has had numerous attacks of venereal disease in all its forms, and is in the excitable, nervous condition which follows prolonged dissipation. He has suffered from stricture for years, and has had numerous attacks of retention of urine, sometimes for as long as forty-eight hours. As far as I understand his history, no one has ever succeeded in passing a catheter, though many have tried. Once, it is true, at a hospital in India, under chloroform, an instrument was said to have been passed, and he woke with it tied in; but as it gave rise to much bleeding, gave him great pain, never drew off any urine as far as he saw, and was removed in an hour or two, there can be no reasonable doubt that it was passed down a false passage; and this is the more probable, as there is a large and long false passage leading between the bladder and rectum. The existence of this false passage was one of the great difficulties in the case. The other was the extraordinary tendency to spasm—a tendency which was only very imperfectly obviated by anæsthesia. He took both ether and chloroform very badly, with great jactitation and congestion, and, after all, could not be got so fully under their influence as to altogether suspend the spasm of the walls of the urethra when the instrument came down to the stricture. He was treated with the greatest patience; prolonged rest in bed, and all the other requisites for the treatment of stricture above enumerated were prescribed. A small bougie was frequently passed as far into the stricture as it would go, and after it had remained there for some time the house-surgeon or the patient himself would try to get it in further; but all was of no avail, and it became necessary to choose between perineal section and the abandonment of the case. The patient

had now been in hospital over two months, and repeated attempts had been made to pass the catheter under anæsthesia. Of course all varieties and sizes of catheters and bougies had been tried. Now, it was true that the patient had not suffered from total retention since his admission, but he made water with great difficulty and in a very small stream, and had occasional attacks of cystitis. In one of these attacks he had consulted a surgeon at the Cape, who not being able to pass a catheter, and hearing that he was coming to England, gave him a letter to the gentleman who sent him here. It seemed certain that further delay would only expose him to more suffering and to the risk of further disease of the urinary organs. Accordingly, on May 25, the operation of perineal section was performed. An attempt was made to pass Mr. Wheelhouse's staff, in the hope that by that means the mouth of the stricture could be brought into view; but this was found impossible. Though the patient was as fully under the influence of chloroform as it was found possible to bring him, the point of the staff was arrested a considerable distance in front of the stricture. Accordingly all guides were dispensed with. A free and deep incision was made in the central line of the perineum, and the finger placed on the subpubic ligament. Then, the urethra having been opened behind the stricture, a female catheter was passed into the bladder to serve as a guide. A large catheter was passed as far as it would go. The strictured portion of the urethra was then freely incised until the catheter passed into the wound. Mr. Wheelhouse's gorget was then passed into the bladder, and a large gum catheter introduced and tied in. No unfavourable symptoms followed. The gum catheter was kept in position for about a week, and was then changed under anæsthesia. This, however, was very troublesome, for the false passage was still unclosed, and it was found difficult to pass a gum catheter without breaking up the wound and introducing a director into the bladder, though a silver catheter would pass with little trouble. Accordingly, on June 19 the use of the permanent catheter was given up, and a silver catheter (No. 10) is now passed from time to time with ether, as the patient's extreme nervousness renders him unable at present to submit to instrumentation without anæsthetics. The wound is rapidly closing; the patient passes water freely, and very little comes by the wound. At the present date (July 5) the case is going on well, but the patient cannot tolerate an attempt at catheterisation without an anæsthetic. The false passage appears still open, at least from the urethra, for the point of the catheter constantly hitches in some obstacle. This being surmounted, it passes readily and without obstruction into the bladder. In August he was discharged from hospital quite recovered.

So far for the outlines of this very difficult case. A few points require further comment. As to the necessity for perineal section, I need not add anything to what I have just said. Years of trial by various persons had proved, not indeed the impossibility, but the great improbability of cure by any milder measure. Then what operation to select? That of simply opening the urethra behind the stricture seems to be in vogue at Guy's Hospital, where it goes by Mr. Cock's name, though it seems to have been practised and recommended by Mr. Simon some years before Mr. Cock's paper appeared. This is, in fact, the first step of the operation I performed, and, as you might have seen, it is a very easy and rapid process. But I do not think it a very satisfactory one. I do not see that we complicate matters much by the subsequent division of the strictured urethra, while we thus give the patient a good chance of regaining natural micturition and emission; and surely this is worth some risk, even if there be risk. I have known men to regain perfectly natural functions after perineal section, and to remain for years in a perfectly natural state. Thus I met by accident some time since a man on whom I had performed perineal section for stricture several years before, and who assured me that he was in perfect health, passing a catheter occasionally as a precaution, but with no apparent need of it. And we often see the same thing even after complete rupture of the urethra. The opening of the urethra behind the stricture is not usually difficult. I show you here a dissection (made for me by my dresser, Mr. Edgelow) of the pubic arch with the prostate and urethra. You will see the sub-pubic ligament, and will observe that if the left index-finger is rested on the sharp edge of the ligament, and the knife inserted just below it, it will almost infallibly open the urethra, and the dilated condition of the urethra behind the stricture which generally exists, renders this still more certain. In fact, with a little practice, the surgeon may make tolerably sure of opening the urethra behind the stricture, and, having done so, there is no great difficulty in passing a catheter from the meatus through the wound into the bladder.

But is that catheter passed along the track of the urethra? Usually, I think it is. There are, it is true, cases of severe and extensive stricture in which the surgeon may quit the course of the urethra altogether, and make a false passage alongside it, leading by the side of the natural urethra. Such a case I once myself dissected, but they are, I think, purely exceptional. The error could only occur if the obstruction occupied a large extent of the canal, and that is in itself unusual. However, it must be admitted that the operation is liable to this uncertainty in the most complicated forms of stricture. But I think we

are entitled to say that this is not sufficient to form a radical objection to the proceeding. All operations for obstinate stricture are dangerous and uncertain, for the patients are generally broken in health by a life of debauchery; but in cases such as that which I have brought before you, I think the old operation of perineal section (*la boutonnière*) is, on the whole, preferable to its modern modifications. What the value of Mr. Wheelhouse's suggestion for its performance may be, my present experience does not enable me to judge. In this case the peculiarities of the case rendered it impossible to pass the staff. In the only other case in which I tried it, the soft parts in the perineum were so thickened and indurated that I could not bring the urethra into view.

The after-treatment of the case is, I think, best managed by the prolonged retention of an open gum catheter—*i.e.*. a catheter furnished with an india-rubber tube going into a vessel at the foot of the bed—by which arrangement the urine is diverted from the wound. In ordinary cases there will be found no difficulty in changing this catheter every four or five days (under ether or laughing-gas if the man is very sensitive to pain); and when the wound is healed or fairly advanced towards healing, the patient may be allowed to get up, and the catheter passed as often as is found necessary. He must then be taught to pass the catheter himself, and warned never to omit this precaution for more than a week so long as he lives. Very possibly the precaution may in some cases be superfluous; but the passage of a soft instrument gives little trouble and can do no harm.

I repeat that in the most obstinate and difficult cases of stricture (and few could be more obstinate and difficult than this case) there is a fair chance of complete restoration to health by this operation, though the operation is a dangerous one and not always easy, the after-treatment protracted and painful, and the prognosis in the class of patients on whom we usually operate always uncertain.—*Med. Times and Gaz., Feb.* 10, *p.* 147.

DISEASES OF THE SKIN, ETC.

59.—NOTES ON THE TREATMENT OF ULCERS.

By JAMES WHITSON, M.D., &c., Surgeon to the Dispensary of Anderson's College, Glasgow.

An ulcer may be defined as any breach of, or solution of continuity in the skin, other than a recent wound; and the treatment of these is interesting and instructive to the patient observer. In the carrying out of this, our great aim is to make the sore a healing one, and ulcers occasionally fail to progress

satisfactorily from excess, as well as from defect of action, while those which are syphilitic are often tedious even though the patient is put under the influence of specific remedies. Varicose ulcers, due to a weakened or dilated state of the veins of the leg, are extremely common, and the callous ulcer with its high margins and depressed centre are frequent inmates of most large hospitals.

Healing begins at the edges, and is centripetal in its action. In this way the healthy sore is distinguished by its blue periphery and even surface. In its treatment little is required except a dressing of slightly damp lint, careful bandaging, and a studied position and rest of the limb. In carrying this out, however, there are one or two points which require attention, and which materially accelerate complete cicatrization. The lint should be accurately cut to the size of the sore. It should not overlap the sound skin round its margin, for by this means the tender granulations are kept in too moist a state for successful progress. The water-proof tissue which is put on above should extend slightly beyond the dressing in order to prevent evaporation, and the whole is retained in its place by means of an ordinary roller of calico. Careful bandaging is essential so that the pressure may be applied equally all round, and no "reverses" should be put on over a sore, as they bear unduly on one part.

Where the ulcer is defective in its action we must take steps to increase it. If the edges are high we must depress them to their proper level, and there is no more effectual method of accomplishing this than by blistering. The Emplastrum Cantharidis, Liq. of Smith, is a most convenient vesicant, and it should be painted over the elevated margin by means of a camel's hair pencil. If necessary, it can be repeated in the course of a few days. At the same time the dressing should be dipped in a stimulating lotion, and of these we have several to choose from. One of the best is the potassio-tartrate of iron, which acts constitutionally as well as locally, and it may be used of several degrees of strength, as from ten to thirty grains to the ounce of water. Hay's wash, which consists of two grains of sulphate of zinc to a like quantity of water (along with lavender and rosemary), as well as a solution of hydrate of chloral (grs. x. to ℥i. water), are favourite applications with many surgeons. Black wash, the lotio nigra of the Pharmacopœia, is generally approved of in syphilitic cases, but care should be taken to shake the bottle in which it is kept before using it, as the calomel from its weight sinks to the bottom and if undisturbed the mixture when poured out will be quite clear. The lint should not be applied in too moist a state, as it is then incapable of absorbing the discharge, and the sore

will consequently become bathed in it. It is best to renew the dressing twice daily, and when doing so, if it is adherent to the granulations never use force in its removal, but soften the adhesions by means of a few drops of water from a syringe.

The sprinkling of iodoform or calomel over specific sores, combined with a dry dressing, is seldom without good effect, and undermining of the edges is a frequent characteristic of this class of affections. When such is the case the burrowing sinuses should be slit up without delay in order to induce healing from the bottom.

The action of the exuberant sore must be repressed, and sulphate of copper or nitrate of silver will generally be found efficient agents for bringing down its surfaces to the level of that of the surrounding skin.

In treating the callous ulcer a pretty vigorous line of procedure is necessary. These are generally situated on theleg, and are not always easy to cure. The edges are invariably high, with a correspondingly depressed centre, and their condition is in most cases aggravated by the patient going about too long, and by insufficient dressing and bandaging. The elevated margins must be freely blistered until the whole extent of the granulations is reduced to the same height, while a stimulating lotion ought to be applied to the sore, and great care exercised in the bandaging of the limb. In the treatment of most ulcers an occasional change in the dressing is beneficial, any one remedy losing its effect after a period of constant application. It need scarcely be added that strict attention to the state of the constitution is always of the utmost importance, and if the secretions become in any way deranged immediate steps must be taken to regulate them.

I have frequently found marked benefit resulting to an ulcer from strapping. The strip of plaster should extend completely round the leg, and it may with advantage be put on both above and below the affected spot, care being always taken at the same time to see that the circulation in the limb is in no way compressed or impeded. Another method which I have often seen productive of good is the application of a splint, and it should possess a foot-piece in order to prevent any movement of the muscles of the part. In very slowly healing ulcers the incising of the skin round the sore, at a short distance from its edge, is a method which generally assists cicatrization; and in some chronic cases which resisted nearly all treatment, I have on more than one occasion found this procedure to result in improvement. The reasons for this are not far to seek, the chief being the relief of tension and the freeing of the blood-vessels from peripheral pressure, while the approximation of sound skin must tend to shorten the period of reparative action.

A wedge pillow may be placed under the limb, as it favours the return of blood to the heart, and ulcers in a congested state are disadvantageously circumstanced for satisfactory progress. It frequently happens that patients come into hospital in this latter condition owing to a long continued debauch, and in these cases the best local application for the first few days consists of a lead and opium lotion, along with absolute rest and elevation of the limb. The bowels at the same time ought to be freely opened by a mercurial purge, repeated if necessary before proceeding with the ordinary curative measures. Recently I have now and then treated ulcers antiseptically, and the results in all instances were good. The cases were by no means favourable ones, so that the test might be considered a satisfactory and encouraging one. The absence of all irritation, the porous nature of the gauze, and the infrequent change of dressing, are all important factors in the restoration of healthy action, and doubtless contributed greatly to the desired end.

On the Continent the application of a specially prepared sand to granulating sores has been tried for some time with success, and, as it possesses the advantage of continuously absorbing the discharge, seldom requires removal, while cicatrization can thus proceed without interruption. The sand is prepared by first of all heating it to a temperature capable of destroying all its organic particles. It is then soaked in a solution of bichloride of mercury and water, the proportions used being one part of the former to one thousand of the latter. After this the mixture is placed in bottles, and can be used when required.

When the cure of ulcers is complete it is well to give the affected limb steady support by means of a bandage, and for this purpose the india-rubber ones of Martin are admirably adapted. They are preferable to calico, and while accommodating themselves readily to any muscular movement, are at the same time easy of application and agreeable to the wearer. They should always be put on before the patient gets out of bed in the morning. In this way the parts are not likely to become congested, the pressure of blood on the veins is considerably lightened, and the liability to the formation of fresh sores materially diminished.—*Practitioner*, *Jan.* 1883, *p.* 20.

60.—CURE OF ABSCESSES ABOUT THE NECK WITHOUT CICATRIX OR OTHER DEFORMITY.

By F. J. B. Quinlan, M.D., M.R.I.A., Dublin.

Everyone has experienced the trouble and difficulty of curing abscesses about the neck, whether strumous or caused by the irritation of a diseased tooth, without deformity. If the abscess be allowed to burst a most disfiguring cicatrix is the result;

and the same consequence, too, often attends the most carefully performed incision. Occasionally a very deep incision made with a sharp narrow and pointed knife is successful, but not more so than the plan adopted in the following two cases.

Kathleen D——, aged eighteen, of Rathowen, co. Westmeath, was admitted into St. Vincent's Hospital at the beginning of the present winter session suffering from a large and painful swelling of the left lower jaw. There was considerable heat and pain on pressure, and the mouth could not be opened. In the centre of the swelling very deep-seated fluctuation could be felt. Spirit lotion was applied on lint until October 5th, when the purulent matter could be felt a little more than half an inch from the surface. A thin curved needle, about three inches in length, was threaded with fine silver wire, mounted on a handle, and passed deeply into the swelling from above downwards so as to admit of drainage. The wire was thus got through the abscess, and the ends were tied together outside the skin. Spirit lotion was now applied on lint and changed three times a day. From the first there was a small quantity of pus on the lint, and this increased every time until in about a week the abscess was entirely drained. A watery discharge now ensued for three days longer, when the edges of the little wounds began to pucker in. The wire was now removed, and a compress applied over the site of the abscess, and between the two openings, which shortly afterwards closed up, leaving two cicatrices, each about the size of the head of a large pin. These cicatrices were at first red, but gradually became white. She was now able to open her mouth, and it was found that the diseased stumps of the second bicuspid and first two molars remained in the alveoli. These were removed, and she shortly afterwards returned home, all swelling gone and with no appearance of the disease, except the two minute cicatrices already mentioned. She is now in excellent health.

Annie D——, aged thirteen, a pupil in a large boarding school, was shown to me on Dec. 5th, 1881, with a swelling under the jaw, the result of a diseased tooth. It was in a much earlier stage than the former case, and, while fearing it was too late, I ordered three leeches to be applied, the bites to be allowed to bleed as long as they would, but no stuping or poulticing. When the bleeding was over I directed mercurial ointment to be gently rubbed in with the point of the finger three times a day. This was, of course, for the purpose of producing resolution, and might have succeeded. Unfortunately the mother of the child, who had a great prejudice against mercury in any shape, prevented the latter part of the treatment, and, as a result, suppuration occurred. The same plan was adopted, the matter being got at a good deal deeper. The

result was perfectly successful, and I mention the case because, shortly before last Christmas, I was shown the same patient suffering from a slight attack of bronchitis. Curiously enough, it was on the anniversary of the former operation, and I was positively unable to tell by ordinary external inspection on which side the abscess had been. She pointed out the spot, and I could then see with a lens the two cicatrices, reduced by contraction, to minute white points. The offending tooth had, of course, been extracted at the time of the operation, shortly after the removal of the silver wire.

The great point in this procedure is to introduce the little silver seton immediately after suppuration has commenced, and while the matter is still at least half an inch from the external surface. If it be allowed to come nearer, absorption of the areolar tissue will occur, and will leave a hollow over the site of the abscess. Nearer still, the true skin may be injured, and then there will be, in addition to the hollow, a red mark, which will last for a long time. If the seton be introduced exactly as in these two cases, there will be practically no mark whatever. The needle is best introduced mounted on a temporary handle, like that which holds the mirror of a laryngoscope. If held in the fingers it is hard to get the necessary depth. In the case of D—— the abscess was exactly over the facial artery; but there would have been no danger in introducing the needle double the depth, for the amount of plasmic matter effused lifted the part well away from that vessel. From first to last there must be neither poulticing nor stuping. This method has never failed in my hands, and I could give other cases, but they would be mere repetitions. Miss D—— was the first instance in which I used it, and I merely mention her case to show the satisfactory state of things a twelvemonth afterwards. One of my other cases was done with strict antiseptic precautions, but, as far as I could see, without any corresponding benefit.—*Lancet, Jan.* 20, 1883, *p.* 94.

61.—ELECTRICAL TREATMENT OF ENLARGED GLANDS.

By Hercules H. MacDonnell, M.D., M.Ch., B.A.Dub., Surgeon to the County Louth Infirmary.

I am desirous of bringing under notice the treatment of an affection only too well known, and which occasions considerable anxiety to the general practitioner when met with, not only on account of the great difficulty sometimes felt in removing it, but also the unpleasant cicatrices and other complications which result. I refer to enlarged glands, whether having an element of struma as a factor in their production or being essentially adenoid cell proliferation. The development of a gland con-

sisting of a local hyperplasia, or development of diverticula from the saccular tubules of the gland and secondary proliferation of the enclosed epithelium, any subsequent degeneration of this epithelium gives rise to caseous masses in the growth. There is frequently a fibroid capsule enveloping the entire structure, and I cannot help thinking that the rapidity of growth and infection of neighbouring glands is modified and retarded by its thickness and vitality.

There are three well-defined stages in this affection, the third of which is that of disintegration, suppuration, and its concomitant evils. My object is to bring under notice the details of a treatment which is most successful in anticipating this degeneration and guiding to a satisfactory termination this most unpleasant affection.

You are doubtless familiar with a treatment adopted by Mr. Golding Bird and termed the electrolytic caustic method. In this his object is to remove the gland by permeating it with chloride of zinc. This he effects by passing a zinc electrode into the substance of the gland, the silver electrode inlaid on the previously blistered surface. A battery with its circuit completed by a copper wire attached to both is thus formed, the exciting fluid for which is the serum of the blood, the chlorides of which are taken up and replaced by the chloride of zinc. This treatment is most effectual, but can only be applied when caseous or other degeneration has actually occurred; therefore any well-conceived line of treatment which anticipates that stage must be welcome to all. I shall, therefore, briefly call attention to the following suggestions, and give notes of four or five cases out of many in which I have found them thoroughly satisfactory.

Having selected the gland or mass of glands you purpose treating, have the surface well cleaned and wiped over with a solution of salt. Apply the negative pole of a Leclanché battery, having two cells connected, over the most prominent part, and the positive about three inches apart; keep moving the positive reophore in a circle round the negative quite slowly, till the electrical stimulus has been sufficiently applied. Usually five to six minutes is long enough. On the first occasion two cells are enough, as it accustoms the tissues to the action. On the succeeding applications the effect of additional cells may be tried; but should there be the slightest appearance of inflammatory action, as evidenced by a bluish-white tint under the negative reophore, a couple of cells must be at once disconnected, or the application discontinued on that occasion. I have never used more than eighteen cells continued for three minutes, and have found that from eight to twelve cells give the most satisfactory and rapid results. The length of each

application varies for different individuals. In some patients three or four minutes twice daily seemed to suffice; in others a longer application only once answered better. Even different glands, or masses of glands in the same individual, progressed more rapidly under varying conditions of length, strength, and frequency of application.

Case 1.—Mr. Q. D., æt. 19, suffering from an enlarged submaxillary gland the size of a large walnut. Has resisted treatment by iodine, blistering, cod-liver oil, iron and phosphorus, &c. In Oct. 1879 began by applying once daily four cells of a Leclanché battery, gradually increasing at intervals of two days to ten cells. After the second application the gland began to diminish in size. The battery was applied twenty-five times in all, when the enlargement had totally disappeared.

Case 2.—Mary G., æt. 20, had a mass of enlarged glands at angle of jaw on either side, extending up behind the ears. On Jan. 3, 1880, electricity was applied four times a week, gradually increasing the strength from two to ten cells. At the fifth application the glandular mass on the right side began to decrease, and at the seventh, that on the left. There were thirty applications in all, when only a faint enlargement could be detected on the right side. This subsequently disappeared.

Case 3.—Catherine B., æt. 23, admitted to Co. Louth Infirmary on March 2, 1880. There was an enormous mass of enlarged glands on the left side of the neck, also a solitary one the size of half an orange on the right side. These had lasted for a year, and resisted treatment by iodine, blistering, and cod-liver oil. Four cells were applied twice daily for three days, each time for five minutes. At the sixth application diminution was perceptible, and at the end of three weeks the masses had almost disappeared. She returned home on April 1, and on the 21st there was no trace whatever of the glands.

Case 4.—Mary McH., æt. 22, was admitted to Co. Louth Infirmary on April 29, 1881. The left submaxillary gland was enlarged to the size of a hen's egg. She had been previously treated with external applications of iodine, blistering, &c. Eight cells of a Leclanché battery were ordered to be applied three times daily for four minutes, five days subsequently increased to twelve cells. On May 8, 9, and 10, sixteen cells were applied; on the 11th, 12th, and 13th, only twelve cells; on the 14th she had two applications of twelve cells and one of eighteen cells; on the 15th and 16th, two applications of twelve cells. The tumour all this time was steadily decreasing, and she left on the 31st.

The foregoing cases are fair examples of what a steady application of the continuous current will effect in these troublesome cases. It has struck me that fair-skinned patients bear a more

heroic line of treatment better than dark ones, and re-act more quickly to the electrical stimulus. I have no doubt whatever that, if applied in the manner indicated, electricity will not fail to give that complete satisfaction which I have experienced.—*Medical Press and Circular, Jan.* 17, 1883, *p.* 50.

62.—RAPID SUCCESSFUL TREATMENT OF ERYSIPELAS.

By Richard Barwell, F.R.C.S., Senior Surgeon to Charing-Cross Hospital, London.

Within the last two months I have had three cases of erysipelas here and two elsewhere, the treatment of which has been peculiar, while its very rapid success must cause much thought and consideration upon the relationship between its *modus operandi* and the morbid cause of erysipelas. I will shortly relate these cases, only premising that the plan I adopted in Case 1 is not that to which I desire to direct your attention—it merely led me to it.

Case 1.—Miss ——, aged thirteen, was brought to me in October, 1882, with a diseased metacarpal bone. I found a large portion of the shaft necrosed and the hand a good deal swollen. On Oct. 19th I removed the sequestrum (the greater portion of the shaft) through an incision a little under an inch and a half long. On the 23rd, a spot of erysipelas, with the usual dusky hue and hard boundary of cessation on the skin, appeared—not at the wound, but a little above the ulnar styloid process; this was rapidly spreading. I painted the part and surrounding healthy skin with glycerine, and dusted over it boracic acid, making a tolerably thick crust, directing that more be applied wherever it flaked off. The inflammation disappeared in nine days—viz., she was well on Nov. 1st. But during the treatment I found that the paste did not keep on well, becoming detached in flakes, and that where this detachment took place the inflammation did not clear up as elsewhere, although there was on the skin a considerable dusting of the antiseptic boracic acid. This led me to think of some more sticky material, and my memory reverted to twenty years ago, when I think it was Mr. French, then of Marlborough-street, who told me he used white lead paint. I confess I attached at the time little importance to the matter, and it had apparently faded from my memory. However, since the above case seemed to show that it was not the "germicide" drug, but the exclusion of air, which produced the benefit, I determined to try a thick coating of paint on the next occasion. As chance would have it, this soon occurred.

Case 2.—George F—, aged six, had fallen, five days before I saw him, on some sharp object, cutting his knickerbocker stockings

and inflicting a pretty deep wound over the patellar tendon; this had been poulticed. On Nov. 22nd, at 6.30 p.m., I was sent for to see the child. I found the wound dry and harsh-looking. Over the tendon of the biceps and in the popliteal space was an erysipelatous blush, with hard sharp edge and rather considerable swelling. The child was fretful, irritable, and evidently in pain; tongue rather foul; temperature 103·2°; pulse 117. I sent for a pound of white lead paint, and painted the whole part inflamed and a little beyond, leaving out the wound, which was dressed by pressing into it a little cotton-wool soaked in a 5 per cent. solution of boroglyceride. Two grains of grey powder and five of rhubarb to be given at once. —23rd (9.30 a.m.): About three-quarters of an hour after I left the child had become quiet, and asked for some bread-and-butter, which with a cup of milk was given; he then fell asleep and passed an excellent night. Bowels have not acted. Temperature 99·7°; pulse 90. Where in a few places the paint had cracked it was renewed. There is nothing more to remark in the case. The temperature became normal the next evening, and did not again rise. In seven days the epidermis was desquamating, bringing away the paint gradually and in patches.

Case 3.—George F——, aged thirty-nine, was in the hospital with a fractured tibia. On the outer side of the leg was a wound, which did not communicate with the fracture. On Dec. 12th, having been in the hospital five weeks, he had some shivering, and his temperature rose to 101·8°. On the next morning erysipelas was found in the neighbourhood of the wound; the part invaded was at that time about two inches and a half in diameter. On the 14th, when I saw the man, I found him undergoing a severe attack of cutaneous erysipelas, which by this time occupied the whole leg, but did not extend above the knee. He said he was in considerable pain, and that his leg felt as if it was on a slow fire. Tongue slightly furred; temperature 102·1°; pulse 108. He had already been ordered a purge. I directed his limb to be coated with white lead paint. On the 15th he told me that in half an hour after this application the pain entirely ceased. Temperature normal; pulse 98. In this case my then house-surgeon misunderstood my wishes and removed the paint with turpentine. I saw him the same afternoon, and ordered its reapplication, as the skin, either from the action of the turpentine or by non-cessation of the disease, was a little red. He did unremittingly well.

Case 4.—Jane M'K——, aged five, has pathologic dislocation of the hip, with necrosis about the head and neck of the femur. The sequestra had been removed, and a drain through to the back of the thigh established.—Dec. 19th: She had shivering and vomiting, with a temperature of 103·6°.—21st: Erysipelas

of the thigh and buttock; the whole part painted. In the evening the temperature was 99·4°. She went on perfectly well. In eight days the paint was peeling off, and the skin quite normal.

Case 5.—Robert L——, aged thirty, received a scalp wound over the lambdoid suture of the right side on December 26th. He was treated as an out-patient. On Jan. 15th he came back with severe erysipelas of head and face, which appears to have been going on some days. A purge was ordered.—16th: I found him with his head and face wrapped in absorbent cotton-wool. This was removed. His hair had been already cut from both head and face. Both were much swollen, the eyelids were so large that they could hardly be parted sufficiently to see the eye, the lips and the alæ nasi also were generally swollen. Temperature 103·8°, pulse 114. Complains of much pain. Ordered to be painted; no other remedy to be used.—17th: Mr. Wyborn (house-surgeon) found before painting some boggy patches on the scalp; these he punctured, evacuating pus. I directed that the head as well as the face be now painted. The man, however, is much better, the temperature lower. He is free from pain.—18th: The patient appears convalescent, his temperature is normal, his pulse steady (98), and he is desirous of more food. The man assured me that within half an hour of the application the previous severe pain entirely disappeared.—22nd: Patient very anxious to go out, but I keep him a day or two that the paint may come from the hair.

The temperature chart ran thus:—

January 16th,	Morning	103·8°	.. Evening	102·1°*
,, 17th,	,,	99·8	.. ,,	100·2
,, 18th,	,,	99·9	.. ,,	98.0
,, 19th,	,,	97·7	.. ,,	97·9
,, 20th,	,,	96·6	.. ,,	97.1

* Between these times paint applied.

And so on, the thermometer never rising again beyond 97·3°. I do not know why the temperature was so low, unless it was a personal peculiarity; the man was certainly not feeble.

These cases are, I think, sufficient to show that a very great effect is produced upon erysipelas and its congeners by covering the surface with white lead paint. This is, I believe, entirely due to exclusion of air; the lead of that compound is in an insoluble condition; nor do I believe that the inflamed skin is in a state to absorb any material applied on its surface; by the time that it has recovered the lead is separated from the living tissue by a tolerably thick layer of desquamated and dead epidermis. Moreover, lead as a solution of the acetate has been applied from a very early period, and as far as I know without any appreciable benefit to erysipelas.

I began this lecture by speaking of erysipelas as one of the so-called hospital plagues; it has long been known both in an endemic and epidemic form. Since the germ theory of disease has been propounded this has been considered, by those who accept in full that hypothesis, to be a typical germ-produced disease. It certainly comports itself after that fashion. Nor need I insist upon all the evidence to be found in every medical and surgical work that erysipelas is a blood disease, not merely a local cutaneous inflammation, but an infection by putrescent or other morbid poison of the whole system; that in this particular medical and surgical erysipelas do not differ, although it is certain that the idiopathic form of the malady very often begins in the fauces, while the traumatic variety has a tendency to commence in or near the wound; but that this marks not any difference in the sort or action of poison, but only a diversity in its place of entrance. And yet here, in a number of well marked cases, you see this disease yield to a mere local application—to simple occlusion of air; yielding, that is to say, not hesitatingly or doubtfully, but at once and unmistakably. I will not now take up your time (for clinical teaching should concern itself not with speculations, but with facts) by attempting to show you how this may perhaps happen, but must nevertheless tell you that I am by no means prepared to discard the blood-poison etiology of erysipelas; yet why a general systemic disease should be thus cured by local treatment is very curious, and of course many more trials and successes must be realised before so strange a doctrine can be accepted. Fortunately our opportunities of seeing erysipelas are very few; it may be months before any, even slight, cases occur. I hope other surgeons may soon report to us their experience in this matter.

Since the above was written, I have had, through the kindness of Dr. Bruce, the opportunity of treating a case of idiopathic erysipelas, with equally rapid success. A lad, aged nineteen, presented himself on Feb. 16th, with erysipelas of the face, in its early stage. The fauces were much inflamed, red, and swollen; the inflammation passing forwards along the nares, and up the lacrymal duct, had spread to the ala nasi, as also to the lower eyelid, which were red, hard, swollen, and still were enlarging. Temperature 102·4°; pulse 120. He was sent to bed, a purge was administered, and his face was painted with white lead paint. This application was renewed twice; no other remedy was used. In six days the lad was well. Mr. Wyborn was good enough to examine the urine. It was quite normal. Tested with hydrochloric and with hydrosulphuric acid it gave no precipitate, showing the absence of lead. —*Lancet, March* 10, 1883, *p.* 400.

63.—ON THE LOCAL TREATMENT OF ERYSIPELAS.

By John Kent Spender, M.D.Lond., Physician to the Mineral Water Hospital, Bath.

This subject deserves every attention, and the readers of the British Medical Journal ought to be thankful that it has been so often discussed. But the question of etiology is, perhaps, not always clearly grasped, although it must determine to a large extent the type of the disease, and its successful management. Take erysipelas of the scalp, for instance: a superficial erysipelas is a very innocent thing, and easily controlled; but there is hardly a more dangerous malady than traumatic erysipelas of this part, accompanying profuse suppuration in the connective tissue between the occipito-frontalis muscle and the cranium. Again, if the sanitary arrangements of a private house or of a large public institution be defective, there may be frequent epidemics of facial erysipelas and of enteric fever, either separately or conjointly, as happened in the Somerset Lunatic Asylum in 1879 and 1881.

But if surgical and hygienic agencies can be safely put aside, it may be acknowledged that the local treatment of erysipelas deserves a large amount of professional care and discrimination. Call it by what name we like, it is essentially a spreading dermatitis, which may cause peril by the extent of cutaneous surface involved, or by the degree of constitutional irritation which may be provoked. It is not often that we have a quasi-inflammation so completely under therapeutic command. Iodine may be useful if there be any suspicion of a pyæmic complication; but, for the so-called idiopathic erysipelas, I cannot speak too highly of the free and frequent application of a solution of tannin in equal parts of spirits of wine and water, as recommended by Dr. Braithwaite in the Journal for April 30th, 1881. This solution is quite as beneficial when erythema approaches erysipelas in local and general severity.

I give the bare outlines of two cases.—1. A lady, a little past middle age, had a sudden attack of erysipelas all over the left thigh and leg, after a trivial injury. The general health was tolerably good. About a dozen "paintings" with the solution of tannin were sufficient to drive away every trace of the disease, the swollen skin soon presenting a shrivelled look.—2. A maiden lady in middle life, entrusted to my care by Mr. Clouting of Thetford, suffered from erratic erysipelas on the face, after exposure to cold, in October 1881. The tannin solution was very successful, and a recent letter from Mr. Clouting tells me that the lotion has been frequently used during the last twelvemonth with the same good result.

Tannin completely dissolves in equal parts of water and spirits

of wine; and, when applied to the skin with a camel's hair brush, a delightfully cool feeling follows from evaporation. A proper strength is six grains to the drachm of fluid.

One of the great literary wants of our profession is, a first-rate monograph on erysipelas in its medical and surgical aspects. Just because it has these two aspects, the subject has rather "fallen between two stools," although handled with more or less ability in various dictionaries and encyclopædias. But there are several points in its pathology and treatment on which most medical men would like to have new and trustworthy teaching.—*British Medical Journal, Dec.* 9, 1882, *p.* 1147.

64.—ON THE TREATMENT OF A VERY EXTENSIVE OUTBREAK OF RINGWORM OF THE HEAD, IN A SCHOOL.

By Alder Smith, M.B.Lond., F.R.C.S., Resident Medical Officer, Christ's Hospital, London.

The following is a summary of the management of one of the largest outbreaks of ringworm of the head hitherto recorded. It shows the result of treatment by the compound carbolic acid, citrine, and sulphur ointment (described in *Retrospect*, vol. 81, p. lvii), and by oleate of mercury; also the great value of croton oil, when applied to small places of disease which have resisted treatment for some months.

On May 2nd, 1882, I visited a school consisting of ninety-two children, between the ages of nine and eleven years; and, on inquiry, learned that there had been some chronic cases of ringworm in the school, and that an extensive outbreak of cutaneous and scalp ringworm had recently developed.

On careful examination, it was found that forty-six out of forty-seven boys, and thirty-seven out of forty-five girls, had ringworm of the scalp. Some of the cases were chronic and extensive, others were moderate and more recent, while some were only one or a few days old. By far the majority had body-ringworm as well; but, as the scalp-disease was the cause of the cutaneous outbreak, and the latter was of very secondary importance, we did not tabulate the number of the spots on the bodies, nor the results of the treatment. I may just mention that the cutaneous patches were all cured during the first two or three weeks, either by Coster's paste, acetic acid, or the compound ointment; and that, after the heads were well under treatment, and the clothes fumigated, no fresh spots appeared. This leads me to remark that an outbreak of body-ringworm, in a school, is generally due to scalp-ringworm—probably overlooked.

The first precaution taken on May 2nd was the isolation of the nine unaffected children; and the following preventive

ointment was ordered to be rubbed into all their heads every day: ℞. Hydrargyri oxidi rubri gr. v; hydrargyri ammoniati gr. x; olei amygdal. essent. ℥j.; adipis benzoati ʒj. M.

The infected children were carefully examined, and all the diseased patches were clearly marked by cutting the hair from, and for half an inch, round them; and then all the small and recent places were blistered with glacial acetic acid, containing four grains to the ounce of corrosive sublimate. The large patches, which had probably existed for weeks or for months, were not blistered. The rest of the hair was cut, and kept moderately short during treatment. The heads were then well washed, and the following ointment was ordered to be rubbed into the entire scalp every morning, and into the diseased patches again every evening, while the head was washed twice a week: ℞. Acidi carbolici (Calvert's No. 2) ʒ vj; unguenti hydrarg. nitratis ʒ x; unguenti sulphurin. ʒij. M.

As the outbreak was so extensive, it was impossible at first to isolate the infected children; so they occupied their usual dormitories and school-rooms till September, when those who were not cured were removed to, and isolated in, the infirmary.

On May 12th, we found, on one of the free cases, a spot of recent ringworm, which had probably developed since the last examination; this case was quickly cured. No fresh spots were to be observed on the infected cases, nor did there appear to be any spreading of the original patches. As the body-ringworm was nearly stamped out, all the clothes were ordered to be boiled, baked, or fumigated; and all the dormitories and school-rooms to have sulphur burnt in them, one at a time. On May 26th, *i.e.*, in less than a month after treatment, the school was free from body-ringworm.

After the first fortnight, eight cases, which were evidently chronic, were placed under the oleate of mercury treatment, instead of the ointment; and at the end of eleven weeks (July 21st), twelve more cases, which had resisted the action of the ointment, were also placed on oleate, as it did not appear advisable to continue a remedy longer than three months which was not acting satisfactorily. Later on, croton oil was employed in twenty-three of the cases, either to convert small rebellious patches into kerion, or to remove a few remaining stumps. Many of the isolated stumps were loosened by gently pushing the point of a gold pin, dipped in croton oil, into the individual hair-follicles. Here I may draw attention to this very useful and practical method of removing isolated stumps, which often resist all other treatment for months. The pin should have a fine but somewhat blunted point, and should be carefully pushed a little way into the follicle, by the side of the diseased stump. The point will carry a little croton oil into the follicle, and this

will generally set up sufficient pustulation to cause the loosening and expulsion of the diseased stump in a few days.

In six months, seventy-two cases of ringworm of the head have been cured out of eighty-five, leaving thirteen uncured; three out of the thirteen left the school, and discontinued the treatment on July 21st; five out of the ten school cases will soon be cured, leaving five; one with a recent patch, two that may be cured in a month, and two inveterate cases—very doubtful when they will be quite well.

Taking the above facts into consideration, I think the results are good, and fully show the value of the compound ointment in recent cases, the oleate of mercury in chronic ringworm, and the practical value of croton oil in suitable cases and at the proper time. There is no doubt that very many of these children would still be under care if the oil had not been employed. Yet, as I have before stated, the greatest care must be taken in using it, and some such method adopted as described in *Retrospect*, vol. 81, p. lix.—*British Medical Journal*, *Dec.* 16, 1882, *p.* 1195.

65.—NOTE ON THE PRACTICAL APPLICATION OF SPONGE-GRAFTING.

By D. J. Hamilton, M.B., F.R.C.S.Edin., Professor of Pathological Anatomy, Aberdeen.

As from time to time since I wrote my paper on "Sponge-Grafting" in the Edinburgh Medical Journal, cases have been recorded in which its application for the purpose originally intended has been successful or unsuccessful, I think it may not be uninteresting if I had a few words in addition to what I have already written on the subejct in regard to its practical application.

The first experiments I made were by placing a thick slice of sponge in the wound, sufficient to at once fill up the gap caused by the loss of tissue. There are several objections to this procedure, the chief being that a mass of sponge three-quarters to one inch thick placed over a suppurating wound becomes soaked with pus, and prevents any free drain from taking place. The pus so accumulated is almost sure to putrefy, and so interferes with the process of organisation going on in the deep layers. The danger of contact of such a putrefactive mass with an open wound, although less in the case of one that is granulating, is probably not to be underestimated.

I have, accordingly, generally found that in such cases it is necessary to cut off the superficial parts of the sponge, leaving the thin layer, which had become infiltrated with organising tissue, adherent. All this inconvenience can be avoided by

adjusting the sponge in successive thin layers over the wound. These layers are not more than an eighth of an inch thick, and must be cut in large slices with a perfectly regular surface The only method I know by which this can be accomplished is by means of a freezing microtome. I happen to possess a large microtome suited for the purpose, which I employ for cutting sections of the entire brain. It holds an entire Turkey sponge ; and, when the latter is frozen, the whole mass can be cut into perfectly regular slices, of any desired thinness. Such a layer can be laid with the greatest facility over the wound, so as to fit into all its irregularities. In a few days, the first layer becomes organised. A second can then be placed over this, and so on, a mass of tissue being thus, in course of time, built up. There is no bagging of pus by this method of applying the sponge, and the danger of putrefaction occurring is reduced to a minimum.

Another precaution that is necessary is, to see that, where the wound is granulating, the edge of the layer of sponge does not come into contact with the pellicle of young epidermis at the side. If so, the epidermis will undermine it, and cause displacement. There ought to be one interval of about an eighth to a quarter of inch between the edge of the epidermis and that of the sponge.

Dr. Sanctuary (*vide* Article 67) makes the remark that firm pressure is a *sine quâ non* in obtaining adhesion. I agree with him so far that, when first applied, there ought to be firm and equable pressure all over the surface; but I question, after adhesion has once taken place, whether pressure exerts a salutary influence in promoting organisation. On the contrary, I should consider that the interstices of the sponge would fill up quicker if the vessels of the granulating part had free play. I should almost say that, in the treatment of a granulating wound of the lower extremity, it would be advantageous, when the sponge has once taken firm hold, to allow the limb to hang downwards, and probably to encourage the patient to take gentle exercise. By this latter means, the circulation through the granulation loops will be rendered active, and a certain amount of vascular turgescence is what is really required.

As regards Dr. Ferguson's observation (*vide* Article 66) that, in a case where he applied a layer of sponge, a quarter of an inch thick, to a wound of the calf of the leg, and where, after organisation had taken place, the new tissue proved to be sensitive, I may say that this might quite well be accounted for by small branches of nerves being carried into the interstices of the sponge by the granulation-vessels.—*British Medical Journal, Jan.* 6, 1883, *p.* 7.

66.—ON A MODIFICATION OF SPONGE-GRAFTING.

By James Ferguson, M.B., C.M., Surgeon to the County and City Infirmary, Perth.

The theory of the originator of sponge-grafting (Dr. Hamilton) is that the force of the blood-current causes an upheaval of the vessels lying on the surface of the wound, and that if a proper support is furnished, new growth will take place in the vessels and cause them to reach still higher levels. Blood-clot or fibrinous lymph supplies this support in many natural processes; a piece of sponge may be made to serve the same purpose. The sponge, having fulfilled this mechanical function, in virtue of its organic nature then undergoes disintegration, and may either be absorbed or discharged gradually.

I selected a piece of fine Turkey sponge, and treated it as instructed, by steeping in dilute nitro-hydrochloric acid, then washing with liquor ammoniæ, and finally setting aside in a one-to-twenty solution of carbolic acid. Some weeks afterwards, I chose an opportunity of testing sponge-grafting, though not in a crucial degree. A man was admitted to this infirmary who, having received a horse-kick on the shin three years before, sufficient only to produce an abrasion of the skin at the time, by incredible neglect had allowed a progressive loss of tissue to go on till an ulcer of the following characters presented. The sore extended almost round the calf, its width varying from two to five inches, its surface was sloughing, discharging profusely, and smelling offensively; the skin around was firmly bound down, and presented an irregular but hard margin to the sore. The man had gone on submitting to matters so long that the knee and ankle-joints were fixed at an angle implying great lameness, while the greatest circumference of the calf was nine inches, as compared with thirteen and a half inches at the corresponding level of the other leg. Measures were successfully employed to produce a clean, and latterly a rich vascular, surface. The level of the sore was now for the most of its extent almost that of the body surface, but at one angle a deeply scooped depression, its dimensions about an inch by an inch and a half, presented. The case was admitted under the care of Dr. J. P. Bramwell, under whose supervision the following treatment was followed out. To the part level with the skin, particles of skin were transplanted, and the results, save at a portion to be hereafter described, have been thoroughly satisfactory. To the hollow referred to, I adapted a piece of prepared sponge fully a quarter of an inch in thickness. Three days afterwards there was adherence of the sponge, and any attempt to detach it produced abundant bleeding. I shall not describe the obvious changes which took place

during eight weeks I made daily observations upon the case. Suffice it to say, they confirm in a rough way the statements given with such careful detail in Dr. Hamilton's paper. One fact I may note, however, as favouring a conclusion opposite to that of Dr. Hamilton on the subject of nerve-supply, he not believing in the production of nerve-tissue at an early stage. I pricked the most superficial portion of the new growth from time to time with the point of a needle, and I asked medical friends to do the same, and pain was most certainly felt by the patient on every occasion. At the same time, Dr. Hamilton's far more extended observations entitle his evidence to regard which I cannot claim for mine. Watching the progress of the case from day to day, a fact which strongly impressed me was that a very protracted period is necessary before organisation can encroach to any extent upon the sponge area. Within three days of application, there is firm union between the living tissues and the sponge. We could hardly expect the vessels to continue their invasion at the same rate afterwards. A physical law may have largely to do with starting the process, which afterwards becomes more purely dependent on vital phenomena. In my case, nearly two months after the application, I was able to remove the superficial part of the sponge with the scissors to the extent of nearly half the original thickness before I encroached on newly vitalised tissue.

In the meantime, another opportunity had been presented me for sponge-grafting, and the issues in this case were peculiar. Dr. Graham, of this city, asked me to see a private case of an ulcer of three months' standing, during which period it had baffled all ordinary methods of treatment, and to decide if the new method might be resorted to. The sore lay over the lower end of the tibia; its size was that of a half-crown; its surface was exsanguine and hard; and its edges were tightly drawn down towards its floor. As a preliminary to further proceedings, caustic was used to the margins, and a strongly stimulating lotion applied twice daily for two days to the surface. The latter proceeding had no appreciable effect. Scratching with the end of a scalpel was now resorted to, until a freely bleeding surface resulted. A bit of sponge was then applied, and kept bound on for three days, when it was found to have united. In three days more, the patient complained of much pain extending up the limb, and the appearances of erysipelas were found spreading from the ulcer upwards. The patient declared himself subject to idiopathic erysipelas. These indications were judged as advising removal of the sponge, and the act produced considerable pain and hemorrhage. A subsidence of the diffused symptoms took place immediately. As regards the ulcer, the changes were specially noteworthy. What had been

the type of indolence and obstinacy among such sores was now the picture of healthy action—the surface abundantly vascular and standing well up towards the level of the skin. The simplest dressings were now sufficient to promote repair; and in three weeks from employment of the sponge recovery was complete. What was the cause of this sudden change? Was it the erysipelas, or was it a foreign body having simply an irritant action, or that effecting an action peculiar to itself? In any case, the sponge was a factor in the process, and deserved further employment.

To return to the first-mentioned case. At one portion of the sore under treatment by skin-transplantation, retrogressive changes set in, and became most persistent. Successive sloughs followed one another until there remained an almost circular gap of fully an inch in diameter, with edges abruptly punched out to the depth of over a quarter of an inch, and its floor, which lay close to the bone, of white fibrous-looking structure. Into this depression, after having used a knife to the floor, as in the other case, I fitted a piece of prepared sponge. Two days effected the same union as before. On the fourth day, I forcibly elevated half of the sponge—pain and bleeding again the accompaniments—and snipped off this semicircle with the scissors. The exposed part left presented highly vascular tissue, reaching well up towards the body-surface as compared with the former depth. In four days more, the remainder of the sponge was detached, and its site was seen to be occupied by a level of new tissue, very slightly higher than the other half. The deficiency left below the level of the skin was soon made up by unaided natural processes, small particles of transplanted skin were applied, and in the end nothing remained to indicate a spot treated differently from parts around. The portion of the ulcer to heal most slowly was that where the sponge was originally applied. I should have removed the whole graft here, as in the other instances, but successive shavings from its surface, in the manner before alluded to, had left only the deep portion, which was now incorporated with the new tissue or disintegrated. Skin-grafts, it was noticeable, were slow to attach to this area, but eventually they became reconciled to it, and the result was a complete cure.

My experience of Dr. Hamilton's proposal has thus been comparatively limited, and I leave to wider observation upon it to yield more conclusive evidence as to its general efficiency. I venture to recommend, however, a recourse to the modified or temporary sponge-grafting—if *grafting* is, indeed, a correct expression to apply to the method I have found useful—for at least certain varieties of breach of tissue. — *British Medical Journal, Dec.* 16, 1882, *p.* 1202.

67.—THREE SUCCESSFUL CASES OF SPONGE-GRAFTING.

By T. Sanctuary, M.D., Hayle, Cornwall.

[Dr. Sanctuary, after relating three successful cases of sponge-grafting, makes the following remarks.]

The sponges used were the finest grained Turkey I could obtain. They were boiled in a weak solution of hydrochloric acid for some hours, and then steeped for half a day in a strongly alkaline solution of creasote. Before application, they were rinsed in hot water, and cut in very thin slices; and the wounds were syringed with the same antiseptic solution in which the sponges had been immersed, in which also were dipped the gutta-percha and lint. A single layer of each material was applied in the following order—sponge, gutta-percha, lint—and the whole was covered with a broad strip of India-rubber plaster, applied so as to secure firm pressure. In conclusion, I may remark that I have observed that, unless firm pressure over the sponge be used, the granulations will push the sponge away, instead of growing up through its substance. There are two sets of cases in which I have noticed this adhesion of sponge, where no such adhesion has been intended: 1. Where bleeding cavities have been plugged tightly with sponge, as in excision of the eyeball; 2. Where sponge-tents have been used to dilate the cervix uteri, and have been unavoidably left in longer than usual. In both these sets, firm pressure is probably a *sine quâ non* in obtaining adhesion.—*Ibid, p.* 1202.

68.—ON SPONGE-GRAFTING.

By P. W. Perkins Case, M.B., C.M., Assistant Resident Medical Officer, Whitechapel Infirmary, East London.

My notes furnish six successful cases of sponge-grafting done here, and others have been done by my senior colleague, Dr. Ilott. All I have tried have had the sponge completely transformed before the patient's discharge, with one exception, viz.: a woman who before complete transformation had taken place, went out to see her soldier son on his leaving for Egypt.

Our method is to get the finest Turkey sponge, free from grit, &c., and slice it as thin as possible, soak it in acid nitro-hydrochloric oil for two or three weeks, till all the calcareous and silicious matters are dissolved, then after repeated washings with water, it has a very soft, velvety feel; this, neutralised by washing with liquor ammoniæ and steeping in carbolic acid solution (1 to 20) for twenty-four hours, is ready for use.

A healthy granulating surface is required for it; we prefer that of a burn, especially if there have been loss of subcutaneous tissues. First, we gently scratch the granulations till they

bleed slightly, then place pieces of this sectioned sponge about the size of a shilling on the bleeding granulations and they soak up blood, which, coagulating in the meshes of the sponge, forms thereby a temporary adhesion. The superficial wound-surface, if less than two inches square, we entirely cover with sponge; if more than about two inches square, we cover it about half irregularly, with pieces of that size, and dress it after the Listerian method with oiled silk, six or eight piles of sanitas gauze, gutta percha tissue, and bandage. Sanitas lotion is generally used afterwards at the dressing, it being not so irritating as carbolic acid. The dressings are usually taken down the second day, and the grafts are then found firmly adherent by the coagulum, and comfortable; afterwards dressed every second day; but great discharge requires daily dressing.

Dr. J. Ferguson mentions one of his cases that went wrong. He says: "The patient complained of much pain extending up the limb, and the appearances of erysipelas were found spreading from the ulcer upwards," and "the patient declared himself the subject of idiopathic erysipelas." Three similar cases I have seen, two of them being Dr. Ilott's. I have no notes: but with these, from the seventh to the tenth day the same occurred locally, and in each there was a febrile state of the patient, but, in addition, the sponge with the pus in its meshes was putrefying, and only removal of the sponge by charcoal poultices, complete cleansings, and saline medicine reduced these symptoms; we, therefore, considered each to be local blood-poisoning; and afterwards, when treating a large granulating surface, we placed grafts irregularly (as before mentioned), to obtain the best possible means of cleansing the grafts and lessening the surface of sponge for absorption. I fancy Dr. Ferguson's may have been similar.

Like him, we also found, after the removal of the grafts by poulticing, and a healthy state restored "that what had been the type of indolence and obstinacy among such sores, was now the picture of healthy action—the surface abundantly vascular, and standing well up towards the level of the skin. The simplest dressings were now sufficient to promote repair." What was the cause of this sudden change? The change was, I think, due to the fact, that the loops of capillaries entered the sponge from the granulations below, proceeding upwards, and between the time of the placing the sponge on the wound and its general removal by poulticing, some of the sponge was secured and held on by the granulations; and when the mass of sponge was removed, some very minute portions were left, which were veritable grafts, producing the effects he describes.

The sponge, like a catgut ligature, appears to become completely organised. The graft, partially filled with clot, becomes

paler in colour, and especially so at its edges, then, more of a jelly-like and homogeneous consistence; and at the margin, it will become lost in granulation-tissue having no line of demarcation; this invasion continues from without inwards until the last little central-island of sponge-texture, as such, disappears.

As far as I have observed, sponge does not appear to be transformed into epidermis; if eventually to cutis vera I have, so far, had no means of determining; but in a patient, whose ulcer of sixteen years' existence I grafted on August 9th last (it then being fiddle-shaped, over the front of the lower third of his right leg, six inches long, and about two and three-quarter inches wide at each side, and one and three-quarter inches at the middle), it duly transformed to granulations-sponge grafts equal to about half its superficial area; but I am yet waiting, and I fear in vain, to see the transformation into epidermis completed. Cuticular covering has gone in considerably from the margins, but now proceeds very slowly. In a small wound, and especially in a recent burn, if small, this want of cuticle is rarely experienced over the soft parts, because of marginal growth and drawing on the surrounding skin.

Lastly, I must acknowledge Professor Hamilton's valuable paper on sponge-grafting in the Edinburgh Medical Journal of November, 1881, as the source of my ideas, and would recommend those interested in the subject to read it.—*British Medical Journal, Jan.* 13, 1883, *p.* 51.

69.—ON THE OLEATES AND OLEO-PALMITATES IN SKIN-DISEASES.

By Dr. JOHN D. SHOEMAKER (in *Philadelphia Med. Bulletin*).

Oleates must no longer be considered merely as solutions of oxides in oleic acid, as previously described, but rather as definite chemical compounds or salts, having no excess of either their acid or basic radicals. While the oleic solutions could not have presented, therapeutically, results differing from those of the oxides employed in solution, the oleates themselves present a very different action, by being chemically in a readily diffusible state. To speak of a five or ten per cent. oleate is as absurd as it would be to speak of a five or ten per cent. sulphate of quinine or morphia or atropia, or in fact, any substance having a definite composition. The true oleates, whilst being more efficacious, are of a stable character very different from the oleic solutions in common use; moreover, from containing less oleic acid, they are much less costly, a point not without its practical bearings.

Dr. Lawrence Wolff finds that the best and readiest method

of preparing oleates is by the double decomposition of sodium oleates with solutions of neutral salts. The sodium preparation is made, to begin with, by the saponification of oleic acid with sodium hydrate. A solution of this in eight parts of water is then precipitated by the salt required; the precipitate washed and dried yields the oleate. For the sake of economy, the oleo-palmitates, double salts of oleic and palmitic acids with the metal or base required, may come into use; and, when manufactured from a soap of the oil of sweet almonds, which contains less palmitic acids than other oils, they answer well for dermic medication.

Oleate of Mercury should be made by precipitating a solution of sodium oleate with mercuric chloride, or a mercuric oleo-palmitate may be obtained by substituting the sodium oleo-palmitate. The precipitate readily forms on boiling the solutions. It may be diluted with either the paraffinates or, better still, with lard or lard-oil. It is the best local stimulant and alterative application of all the mercurials. When applied to the unbroken skin, it produces marked stimulation bordering on congestion. When rubbed in over tumours, indurations, and glandular enlargements, or thickening of the skin, it exerts a most valuable resolvent and alterative action. It has many advantages over the old mercurial ointments: firstly, being a chemical compound, it is more readily absorbed by the skin; secondly, being soluble in fats, it has great penetrating power; thirdly, it does not become rancid; fourthly, it is cleanly and economical. It is a most valuable remedy in syphilis. It is rapidly absorbed, leaving only a reddened surface, and there is no staining of the linen. It is quite capable of producing the constitutional effects, so must be used with caution. It is best to employ an ointment consisting of one part of oleate with three parts of lard, and of this a piece of the size of a small marble may be rubbed into the thighs, the limbs, or trunk. It is also useful in the treatment of indurations occurring after abscesses, in excess or deficiency of pigment, either as a disease or as the result of applications, in indolent papules, in obstinate ulcers, particularly the syphilitic, and in cases of enlarged testicle. In the indolent and chronic stages of psoriasis, when the patches are thickened, harsh, dry, and cracked, the application of the oleate does much good, although it is usually necessary to previously remove the scales by alkaline baths, oils, water-dressing, or wet packing. In all forms of vegetable parasitic disease the oleate lightly smeared over the surface will not only kill the parasite on the surface, but will frequently, by its penetrating and diffusive action, pass into the hair-follicles and sebaceous glands, and destroy any fungus that may have penetrated beneath the skin. In

phtheiriasis, the oleate destroys both the parasite and the nits. The oleate of mercury may be employed advantageously in combination with other oleates. A mixture of ten or twenty grains of oleate of mercury with one drachm of the ointment of oleate of zinc is very effective in chronic acne and eczema, especially in the fissured variety of the latter, which is so common on the palmar and plantar surfaces. In all syphilitic skin-eruptions, and in superficial ulcers, one drachm of the oleate of mercury with three drachms of the oleate of bismuth, or the same quantity of the ointment of the oleate of lead, acts quickly and decidedly. The oleate of mercury, in the proportion of one or two drachms to one ounce of oil of ergot, forms one of the best and most efficacious oily applications for loss of hair. It is especially useful when the scalp is harsh and dry, and the hairs look dull and lack their peculiar lustre.

Oleate of Zinc is made by decomposing a sodium oleate with a saturated solution of zinc sulphate, boiling out and drying the precipitate, and then reducing it to an impalpable powder. One part of this, melted with three parts of a fatty vehicle, forms a most useful ointment. The best results, however, have been obtained with oleate of zinc alone, unmixed with any fatty substance. It is a fine pearl-coloured powder, having a soft soapy feel, very much like powdered French chalk. It is a valuable application in all forms of sweating.

Oleate of Lead is obtained by precipitating a sodium oleate with a solution of lead subacetate. The washed and dried precipitate, melted with equal parts of lard, gives an ointment which may be designated the ointment of lead oleate. This ointment is cream-coloured and semi-solid, of the consistence of simple cerate. When applied to the denuded skin, it exerts both a combined sedative and astringent action, and will arrest morbid discharges, protect the surface, and allay irritation. It is more readily absorbed than either Goulard's cerate, or Hebra's litharge ointment, and it is now easily and cheaply prepared. It is of the greatest use in allaying the inflammation and checking the discharge and itching of eczema. It is useful in simple lichen, and in the hard indurated papules in acne of the face and back. It may often be used with much advantage in combination with one or other of the oleates to be presently described. Thus a good combination in many skin diseases is two drachms of the oleate of lead, with one drachm of oleate of bismuth. This has a most beneficial effect in the fissured form of palmar and plantar eczema. When the inflammation and cracking are very deep and severe, and require a marked stimulation, the addition of twenty or thirty drops of the oil of cade, to two drachms of this oleate, will succeed better. In scabies,

an excellent preparation is four drachms of the oleate of lead ointment, with half a drachm of sulphur.

Oleate of Copper is obtained in a similar way to the lead oleate, by double decomposition with a saturated solution of copper sulphate. A 10 or 20 per cent. ointment may be made with either cosmoline, fat, or lard. When applied to the unbroken skin, the oleate rapidly penetrates deeply into the parts, particularly into the follicles, producing slight stimulation. If brought into contact with the broken skin, it coats it with an insoluble albuminate. It is an excellent application for ringworm, and, even in the most obstinate cases, will usually speedily effect a cure. It is a good application for indolent ulcerated surfaces, and in some cases has cured obstinate, hard, and horny warts and corns.

Oleate of Aluminium is prepared by decomposing sodium oleate with aluminium sulphate. The washed precipitate mixed with equal parts of lard, forms the ointment. The ointment thus prepared is semi-solid, dark-brown in colour, and has a most powerful astringent action. It quickly checks all muco-purulent discharges, and is an useful dressing for foul ulcers, sinuses, burns, and scalds.

Oleate of Bismuth can be obtained only by first preparing a crystallised bismuth nitrate, dissolving this in glycerine, and decomposing with this the sodium oleate. It is of ointment consistence, and should be used as thus obtained. It has a pearly-grey colour, and is a soft bland substance. It has an emollient and slightly astringent action, and is a most valuable remedy in soothing and relieving cutaneous irritation. In pustular eruptions, particularly sycosis, the oleate of bismuth, lightly pencilled over the surface with a camel's hair-brush, will greatly relieve the engorgement of the parts, and will often abort the pustules. It is equally efficacious in superficial erysipelas, and in sunburn, and is of much value in chronic inflammation of a portion of the face. It is most useful in gonorrhœa and gleet; the best way being to pass a bougie, covered with the oleate of bismuth, and allow it to remain in the passage for some minutes.

Oleate of Iron is made by precipitating from sodium oleate with ferrous sulphate; on boiling it is converted into ferric oleate, and, as such, may be used either alone or mixed with an equal quantity of fatty base to form an ointment. It is free from local irritation when used topically, but, when brought into contact with an ulcerated surface, it has a mild astringent action. It is readily absorbed, so as to produce the constitutional effects of the iron. A small piece of ointment may be rubbed into the axillæ and groins two or three times a day.

This mode of treatment is of the greatest value when the stomach is irritable, and ordinary preparations of iron are not well borne.

Oleate of Arsenic is derived from the arsenious chloride, made by the cautious saturation of hydrochloric acid with arsenicum. This solution having been obtained, it is mixed with sodium oleate, when the arsenicum oleate is thrown down. In the proportion of twenty grains to an ounce of fatty base, it forms the ointment of arsenicum oleate. It is in this form a soft yellowish ointment, having no action on the skin. When applied to wounds, or ulcerating surfaces, it destroys the tissues to some depth. In lupus, especially the ulcerating varieties, its constant application will destroy cell-infiltration in a mild and comparatively painless manner. It is also used with advantage in the tubercular form of lupus, and in the ulcerating variety of epithelioma. It may be employed, after scraping the surface, to destroy warts, condylomata, nævi, corns, horns, and old granulations. In some cases, it may be combined with opium, belladonna, hyoscyamus, or arnica.

Oleate of Silver, like the others, is prepared by precipitating the sodium oleate with a salt of the metal, a saturated solution of nitrate of silver being employed in this case. The precipitate is washed with boiling water, and then dried, after which it is reduced like the zinc oleate to a fine powder. One drachm of this dissolved in an ounce of fatty material forms a most useful ointment. This oleate, in its natural form, sprinkled over old chronic ulcers, bed-sores, and exuberant granulations, will set up a healthy state in the parts. It is a safe and efficacious remedy in erysipelas, and can be used either round the margins to prevent the inflammation from extending, or it can be applied diluted to the inflamed surface. It checks itching about the anus and in other regions, and is sometimes employed in combination with opium, belladonna, &c.

The oleates of magnesium, lithium, calcium, antimony, tin, &c., are easily prepared, but as yet have proved of little value, as far as dermic medication is concerned.

[Mr. Martindale has recently made the reporter some oleate of zinc, according to the method described in this paper, and it is undoubtedly a very great improvement over the old so-called oleate. As Dr. Shoemaker says, it is 'a fine pearl-coloured powder, with a soft-soap feel, very much like powdered French chalk'. Mixed with thymol (1 in 500), and used as a dusting-powder, it forms an excellent application in many varieties of local sweating. The reporter has used it with much success in the treatment of the night-sweating of phthisis.]—*Dr. William Murrell, London Medical Record, Nov.* 15, 1882, *p.* 449.

70.—ON THE PREPARATION OF THE MEDICINAL OLEATES

By Dr. Squibb (in *Ephemeris of Materia Medica, etc.*)

Various methods of preparation have been advocated, but none so good as the direct union of the acid with the dry base without heating. The preparation should always be either a liquid or a semi-solid which is easily and completely liquefied by the natural temperature of the surface to which it is to be applied, and hence normal oleates undiluted are not applicable to therapeutic uses, but only solutions of the oleates, and these solutions should always be in oleic acid as the solvent rather than in oils, because the acid is more readily absorbed than the oils. In the rare cases where the excess of acid as a solvent of the oleates proves irritant to the skin, dilution with a bland oil becomes admissible. A paper has appeared recently by Dr. Shoemaker (*vide* Article 69), in which it is stated that the oleates as commonly prepared and used are not chemically true oleates, but merely solutions of oxides in oleic acid, and as such will often give negative results. This is a mistake, and is as great an error as it would be to say that mercuric nitrate, made by dissolving mercuric oxide in nitric acid, was not a chemical nitrate, but only a solution of the oxide in the acid. This author also states that the best method of making oleates for medicinal uses is by double decomposition; and this, as a general statement, is also a mistake, as very few oleates are well made in this way, and it is doubtful whether any are best made by double decomposition between solution of oleate of sodium and solution of salts of the bases. At least this is neither the simplest nor the easiest way of making the solutions of the oleates in oleic acid as required for the best and the easiest absorption through the sound or unbroken skin, and it is incorrect to write of preparations made by direct union of the acid and base as "supposed oleates" of "indefinite and unstable character."

The oleates which, up to this time, appear to have been most used, are oleates of aconitia, atropia, mercury, morphia, quinia, strychnia, veratria and zinc. These are for general or epidermic use through the skin, while for special or dermic use in diseases of the skin, oleates of copper, lead and zinc are those most frequently heard of.

Oleates of the more active alkaloids, namely, aconitia, atropia, strychnia and veratria, are usually and properly made of the strength of two per cent. of the alkaloid. The oleate of morphia usually contains 5 per cent. of that alkaloid, while the oleate of quinia is made as strong as is practicable, and usually contains 20 per cent. of the alkaloid.

All of these are very simply and easily made by putting the

weighed quantity of the alkaloid into a mortar, adding a small quantity of the oleic acid, little by little, and triturating until the alkaloid is completely dissolved. The strong solution thus made is then poured into a tared bottle, and the mortar and pestle rinsed twice into the bottle with small quantities of oleic acid. The proper weight is then made up by the addition of oleic acid. No heat is needed, nor should any be used in the preparation of many oleates, but in some of these the digestion is prolonged, and intervals of trituration are needed. All heating has a great tendency to change their molecular constitution. If well-made oleate of morphia be shaken with dilute sulphuric acid, the morphia should be washed out as a sulphate, but it is a singular fact that it cannot all be so recovered as morphia; yet the morphia effect of the oleate is prompt and decided. This appears to show that some change is effected in the alkaloid even by combining it without heat, while if heated the changes are destructive.

The molecular or combining weight of oleic acid is high, namely, 282, but the weights of the alkaloids are still higher. That of aconitia is 645, atropia 239. morphia 285, quinia 324, strychnia 334, and veratria 592. Hence the molecule of the respective oleates would be very complex and very easily split up by any forces tending to decomposition, as heat, light, &c., or by oxidation from undue exposure to air. Hence it is that oleates may not keep well, but should be as freshly made as practicable, and should not be relied upon for their full effect when more than a year old, even if they have been carefully kept in a cool place.

The normal oleates, that is, when the oleic acid is fully saturated by the base—contain the following percentage of the respective bases:

Oleate of aconitia,	about 69·6	per cent. of	aconitia.
Oleate of atropia,	,, 50·6	,,	,, atropia.
Oleate of morphia,	,, 50·3	,,	,, morphia.
Oleate of quinia,	,, 53·5	,,	,, quinia.
Oleate of strychnia,	,, 54·2	,,	,, strychnia.
Oleate of veratria,	,, 67·7	,,	,, veratria.
Oleate of bismuth	,, 22·2	,,	,, Bi_2O_3.
Oleate of copper,	,, 12·7	,,	,, CuO.
Oleate of iron,	,, 11·7	,,	,, FeO.
Oleate of lead,	,, 29·	,,	,, PbO.
Oleate of mercury,	,, 28·4	,,	,, HgO.
Oleate of zinc,	,, 1·29	,,	,, ZnO.

In epidermic medication it must be borne in mind that the skin, in common with the mucous membranes of the primæ viæ, does not absorb with equal facility or rapidity at all times. As was forcibly said by Prof. Meigs in regard to the occasional

inactivity of powdered ergot in parturient women: "There are conditions of stomach in which you might as well put your medicines in a bladder and hang it up on a nail." So there are conditions of skin wherein absorption is slow and difficult, and in all such, hypodermic medication has great advantages, and it is highly probable that the hypodermic use of these oleates would be effective in such cases.—*Pharmaceutical Journal, Dec.* 30, 1882, *p.* 528.

SYPHILITIC AFFECTIONS.

71.—USE OF SALICYLATE OF SODA IN ACUTE ORCHITIS COMPLICATING GONORRHŒA.

By Edward Henderson, M.D., F.R.C.S.Ed., Shanghai.

The marked and speedy relief of pain which follows the administration of salicylate of soda in rheumatic inflammation of the joints has been for some time past a matter of common clinical observation. The success which I have myself had with this drug in treating these affections led me in the autumn of 1880, while in charge of the Shanghai General Hospital, to give it a trial in a case of acute epididymitis occurring as a complication of gonorrhœa. Since then I have had but two satisfactory opportunities of repeating the experiment; I regard, however, the results obtained in these three cases as sufficiently good to warrant further trial of the remedy, and, indeed, entertain the hope of finding by more extended experience that we possess in salicylate of soda a drug which exercises something very like specific influence over the disease.

Case 1 was admitted to the General Hospital on September 13th, 1880. The patient contracted gonorrhœa about three weeks previously. For a week the left testicle had been somewhat swollen and painful. On the 11th the swelling increased considerably, and the pain became severe and constant. On the 12th he suffered a good deal, and was unable to leave his bed; that night the pain prevented him from sleeping, and he had fever. On admission, at 10 a.m., his temperature was 102°. The left testicle was about four times larger than the right, and the scrotum covering it was red, being stretched and glossy in front, and somewhat œdematous below. There was great tenderness. The urethral discharge had disappeared. I thought the tunica vaginalis implicated, and regarded the case as probably a suitable one for treatment by puncture, as recommended by Mr. Smith and others. Salicylate of soda was ordered in twenty-grain doses hourly until the pain should be relieved. At 4.30 p.m. four doses had been taken—namely, at 11.10, 12.10, 1.10, and 4. He had sweated profusely, and the pain

had greatly diminished. At 8 p.m. his temperature had fallen to 100°, and the medicine was ordered to be continued at intervals of four or six hours. He slept well that night without an opiate. On the following morning (the 14th) his temperature was 98·4°, and he felt quite easy while lying down. Pain had altogether left him on the 15th, and the urethral discharge returned on that day. There was no relapse.

Case 2.—A police constable, seen by me for the first time on the morning of April 8th, 1881. Patient contracted gonorrhœa about a fortnight before that date. One of his testicles had been swollen and painful for five days, but, until the morning of the 8th, he had been able to do patrol duty; being then no longer able to walk, he was compelled to go on the sick list as suffering from venereal disease. The inflammation was confined to one testicle, which was between two and three times larger than the other; it was hard and very tender. The scrotal skin was red, but not glossy. The body of the testicle was perhaps affected in this case along with the epididymis. There was apparently no discharge from the urethra. The patient's skin was hot to the touch, and his tongue was coated. He was ordered salicylate of soda in twenty-grain doses every two or three hours according to the effect produced. The testicle was to be suspended in hot water from time to time during the day. By evening six doses of the medicine had been taken, and the pain was greatly relieved. The pain diminished, he said, after the first dose, when he began to perspire. In the afternoon he sweated profusely. On the following morning (the 9th) he was quite easy when lying down, and could bear the testicle to be handled freely. On the 10th he declared that all pain had left him, and asked to resume duty. The testicle was then distinctly less swollen, though still enlarged and indurated. Convalescence was uninterrupted. The urethral discharge returned, but I have not noted the date of its reappearance.

Case 3 was first seen on June 15th, 1882. The patient contracted gonorrhœa a month before that date. For about eight days the right testicle had been somewhat swollen and painful, and at the same time the urethral discharge began to diminish, and finally disappeared. On the 13th the inflammation seemed to be subsiding; but on the 14th the patient walked a good deal, and by the evening of that day the testicle was much more swollen than it had yet been, and the pain was severe and constant. On the morning of my visit he was forced to remain in bed. He said he had had an "awful" night from pain, and had been quite unable to sleep. His temperature was 101·8° F.; pulse 90; tongue clean and dry. His bowels were confined, one small hard motion on the morning of the

14th, and the same on the morning of my visit (15th). The inflamed testicle was at least four times larger than the other, hard, and very tender. The scrotal skin covering it was dull red, stretched and somewhat glossy in front. I ordered twenty grains of salicylate of soda every two hours, avoiding any other treatment, local or by aperients. The medicine was taken at 11.30 a.m., 1.30, 2.30, 4.30, and 7 p.m. After the second dose he had some ringing in the ears and began to perspire. After the third dose sweating was profuse (the weather was warm). At 4.30 p.m., after the fifth dose, the pain was relieved. At seven o'clock I saw him again; his temperature was then 101·6°, and his pulse 84; the pain was much less, and he could bear the testicle to be handled with tolerable freedom. On the following morning (16th) his temperature was 99·4°; pulse 88. He had taken two doses of the medicine during the night. He had slept well, and now complained only of slight pain confined to the upper part of the testicle. The testicle was much diminished in size, and fluctuation could be distinctly felt in front. His bowels were not moved till the evening of that day. Convalescence was uninterrupted and satisfactory.

In further trials of this plan of treatment I would advise that only acute cases be selected, the evidence of that condition being a distinct rise of temperature as ascertained by the thermometer. The dose of the salt should be not less than twenty grains, and should be repeated hourly until at least three doses are taken; afterwards the same dose may be continued at longer intervals.—*Lancet, Dec.* 16, 1882, *p.* 1027.

DISEASES OF THE EYE AND EAR.

72.—OPERATIVE TREATMENT OF CONGENITAL CATARACT.

By ANDERSON CRITCHETT, M.A.Cantab., Ophthalmic Surgeon to St. Mary's Hospital, London.

An argument in favour of adopting the more cautious method by absorption is to be found in the fact, that no necessity exists for obtaining a rapid result, since, one eye only being dealt with, the ordinary pursuits of young life need not be suspended. But, while the above is true concerning a limited number of cases, there exist a decided majority of congenital cataracts in which the partial character of the lenticular opacity suggest an important modification in our operative procedure; and it is to this last point that I desire, in the present paper, to direct attention. The lamellar cataract to which I am now referring is found to differ both in the density of the central opacity, and in the degree to which it encroaches on the more or less clear margin. It is, of course, difficult to lay down any hard and

fast rule for adoption; but practically it will be found that, whenever the transparent margin represents one-third of the diameter of the lens, a sufficiently satisfactory result can be obtained to justify the selection of a modified form of iridectomy, or, in some isolated cases, of iridodesis. Before deciding upon either of these plans, it will be well to ascertain to what extent the vision is improved by complete mydriasis, bearing in mind that the creation of a small artificial pupil will, for obvious reasons, secure a more perfect optical result than can be obtained by the use of atropine. The object in view in altering the shape and situation of the natural pupil is to obtain an aperture opposite to the transparent margin of the lens, and corresponding, as nearly as possible, to the size of a central pupil in a moderate state of contraction. The most convenient situation for it is downwards and inwards. The method which my father has recommended, and which I generally adopt, is as follows.

An anæsthetic having been administered, the lids are separated by a stop speculum, and the operator, steadying the globe with a pair of fixation-forceps, introduces a broad needle set at an angle into the anterior chamber, being guided as to the point of entry by the amount of iris which he desires to remove. If there be an exceptional width of clear lenticular margin, the incision may be made within the corneal area to such an extent that a portion of the peripheral circumference of the iris remains intact; but if, on the other hand, the transparent region be more limited, the pupil must extend further inwards, and this may be accomplished with considerable precision by accurately defining the position of the corneal wound. The broad needle should be introduced at an exact right angle to the cornea, so that it may enter the chamber without travelling unduly between the corneal layers. As the point of the needle is seen within the chamber, the handle of the instrument should be depressed, so as to avoid the possibility of wounding the lens. The needle should be slowly withdrawn, otherwise a rush of aqueous humour may entangle the iris in the wound. The operator then introduces a small blunt hook, known as Tyrrel's hook, also curved at an angle like the needle. The hook is passed in on the flat beyond the pupillary margin, which is caught by inclining the free edge of the hook slightly downwards. Steady traction should then be made towards the wound; but immediately before emergence the hook must be gently rotated, so that its free edge presents a little forwards, to secure an easy exit, and to prevent its entanglement in the corneal aperture. The portion of iris which has been drawn out, and is still held by the hook, should be cleanly excised with small blunt-pointed curved scissors, close to the cornea, by that much to be desired, but not always available, coadjutor, a competent assistant.

If gentle friction be then made with the upper lid over the puncture, the iris floats back into the chamber, and all the elements of a well accomplished iridotomy are secured without its palpable dangers. Nothing further is needed but a light water-dressing and a few days of rest. I may add, that the limitation in the size of the pupil which this method secures, obtains for us a better optical result than can be produced by any iridectomy performed with keratome and forceps.

As I have already stated, the practiceof removing the lens in cases of congenital cataract is, I believe, the plan which is still most commonly adopted; though I am pleased to say that our President, Mr. Vose Solomon, spoke as follows in an inaugural address which he delivered at Queen's College, Birmingham, in 1876. "The plan of making an extension of the pupil by the removal of a narrow strip of iris in cases of congenital cataract, in which there is a zone of transparent lens, must be considered as an important advance; and where this condition of lens is binocular, I am in the habit of acting upon the method named in one eye, and destroying the other by solution." The advocates of the major operation will doubtless claim for it that cosmetically they gain a clear black central pupil, that the result is final, and that by the aid of suitable glasses, an optical condition may be anticipated which is superior to that which obtains by any other plan. The drawbacks which experience has forced upon my attention are: firstly, the serious risks involved; secondly, the disadvantage, especially to the young, of constantly wearing heavy and powerful glasses; and thirdly, as I have noted in numerous instances, the eye that has been deprived of its lens possesses a lower vitality, and is more liable to failure of its functions. I think I may fairly allege that the operation by artificial pupil has the following merits. It is one of great simplicity. and when performed with adequate care and skill, of almost invariable safety. The recovery is extremely rapid, and entails a very brief convalescence. The vision, though somewhat below the most perfect results obtained by absorption, is in most instances sufficiently good for all the purposes of life, and is obtained without the aid of glasses, while the power of estimating distances is intact. The chief objection that may be urged against this plan is the possible extension of the central lenticular opacity to the margin. This may be true of certain cases which occupy an intermediate position, and where radiating striæ invade the transparent portion of the lens. Even here, however, I should not hesitate to give the patient the benefit of the doubt, and of the artificial pupil; since, in the event of a necessity arising for the major operation, we know on the high authority of Von Graefe that a preliminary iridectomy introduces a decided element of safety.

I have, however, no hesitation in asserting that, where a clean and clearly defined limit exists between the central opacity and the transparent margin, the latter remains bright through life. I have seen several cases answering to the above description where more than twenty years have elapsed since my father performed on them either iridectomy or iridodesis, and where the peripheral portion of the lens has retained its absolute transparency. The converse of this has not yet come before my notice. I will not weary you with statistics of cases in confirmation of these views, though the confidence with which I have ventured to state my position is fully justified by the result of my experience in this direction at St. Mary's Hospital. I will, however, allude to one case which I brought before the Ophthalmological Society this year, and which well illustrates the relative value of the rival operations.

Isaac G., aged eighteen, was admitted to St. Mary's Hospital on April 18th, 1882. The cataract in his right eye had been absorbed eight years before, and with a + $3\frac{1}{2}$ lens he saw $\frac{20}{30}$, and with a + $2\frac{1}{2}$ read No. 4 of Jaeger's test-types. He complained that, owing to the necessity for using a strong glass, he was unable to procure a situation, or to earn his living. In the left eye, where he had a lamellar cataract, I performed a small iridectomy downwards, and inwards, with the result that he without a glass sees $\frac{20}{70}$, and reads Jaeger 4, and has obtained a situation as a domestic servant. I by no means claim any element of novelty or originality for the foregoing observations; but I have felt, and strongly feel that the merits of the particular mode of treatment which I have here advocated, have not hitherto been generally estimated at their true value; and I venture to hope that this plan may in the future obtain a more extended trial, since I am well assured that it deserves to supersede, in a large proportion of cases, its older and more firmly established rivals.—*British Medical Journal*, *Dec.* 23, 1882, *p.* 1241.

73.—ON DISLOCATION OF THE LENS, WITH REMARKS ON THE OLD OPERATION OF COUCHING.

By Edwyn Andrew, M.D., Surgeon to the Shropshire Eye, Ear, and Throat Hospital, Shrewsbury.

Most writers on injuries to the eye, producing dislocation of the lens into the vitreous body, are inclined to take a grave view of the condition; but, from a number of observations made, my opinion is much more favourable; in fact, I have been surprised, under such circumstances, to notice the great power of recovery the eye possesses, and the excellent vision obtained by the aid of glasses, with little or no treatment besides rest. In the

majority of such favourable cases, it must be allowed that the lens has been dislocated with its capsule entire, and the external coats of the eyeball left intact. When, from greater violence, other lesions are added, such as rupture of the lens capsule, or rupture of the external coats, with admission of air containing the dreaded germs of the present day, the prognosis becomes more and more unfavourable in proportion to the addition of these evils; recovery becomes much more slow, as the iris is generally implicated; the vision obtained is less perfect; much more active treatment is required; and, when these evils are conjoined with admission of air, often general inflammation is set up, requiring immediate extirpation of the whole eyeball.

It seems, then, to me, that simple dislocation into the vitreous body of the lens encased in its capsule causes little or no irritation, and most commonly has a favourable termination. Without referring to the spontaneous dislocation of the lens, and usually the very slight disturbance therefrom, the two following cases are recorded, out of several others, in support of the above views.

Case 1.—G. J., aged 70, a labourer, whilst breaking stones, received a severe blow on the right eye from one of the fragments; the pain produced was only temporary, but vision was so impaired that he was unable to continue his work, the left eye being previously affected with incipient cataract; after five weeks' rest, therefore, he presented himself at the Eye Hospital.

On examination, the right pupil was found round and contractile; the iris was flat and vibratile; tension normal or *minus;* the cornea was clear; there was little or no conjunctival injection; the disc was clearly seen; there were no moving bodies, and the lens was distinctly visible, resting on the lower surface of the eyeball. With + 2½ and 4, he could read for near sight No. 4 of Jäger, and for far sight $\frac{2}{70}$ of Snellen.

As there was no sign of any irritation, merely a placebo was given. Six months later the right eye was perfectly quiescent; the media were clear, and vision same as before. In the left the cataract had advanced. He was told to report himself, should he feel the slightest uneasiness in his eyes, but he has not done so for the last eighteen months.

Case 2.—J. P., aged 73, came in March last for injury to his right eye, also from stone-breaking, six months previously. There was detachment of the inner half of the iris, forming a double pupil, not contractile. The iris was slightly inclined backwards, and very vibratile; tension was normal, or slightly increased; the cornea was clear; there was no injection. The lens was partially dislocated outwards and backwards; the inner margin was very visible, from having a fringe of pigment

adhering to it. It was slightly hazy, but the disc could be seen through it indistinctly. With + 10 letters of 16 could be made out with the greatest difficulty, but for distance $\frac{0}{0}$. He said he had lost the sight of his left eye for twelve years from a slight injury. On examining this left eye, there was a small central contractile pupil with a vibratile iris. Tension was normal, the media clear, and disc distinct; there were no moving bodies; and the lens was found dislocated to the lower fundus, movable, and apparently anchored in this position by the lower part of the suspensory ligament. The edge was readily seen by a black circular margin; the capsule seemed shrivelled with a white centre (nucleus?). With cataract glasses, to his astonishment, he could make out No. 1 Jäger and $\frac{20}{30}$ Snellen. Seeing the happy result of accident in his left eye, I determined to imitate the proceeding in his right eye. He was placed under ether; a small opening was made near the corneal margin with a narrow bent flat needle, and an instrument made of a piece of stiff wire, having a rounded point, and a line to half a line of its extremity bent at a right angle, was introduced, and the suspensory ligament torn through except at its lower part. The instrument was now placed sideways in front of the lens near its upper margin, and pressed back so as to push the lens downwards and backwards in its capsule to the lower fundus of the eye. On recovery from the ether, the patient at once noticed improvement in vision. During the evening, and also the next day, there were some general injection, slightly increased tension, and also slight vitreous haze; but the tension soon disappeared by the application of eserine twice a day, and the haze gradually diminished, so that at the end of the week he could read, with cataract glasses, 14 Jäger and $\frac{20}{70}$ Snellen. At the end of a fortnight, the disc was almost distinct. Words of 4 Jäger were read with difficulty and $\frac{20}{70}$. From the condition of the iris, perfection of sight in this eye would be simply impossible.

Do not these and other similar cases show the performance of the old operation of couching in the roughest manner, but producing results, as we see in one instance mentioned above, and after a period of twelve years, so good that it is rarely equalled by the best mode of extraction? Do they not point out that the almost universal condemnation of this old operation of our forefathers requires to be modified; for, disastrous as the results of couching have been, and having succeeded to the hospital practice of a surgeon who chiefly depressed, I speak with some authority, and believe that, in suitable cases, these bad results have been rather due to laceration of the capsule than to the forced dislocation of the lens.

Putting aside a large number of cases for which couching is

quite unsuitable, and confessing that I have only had the courage to carry out my opinion in a few cases, still, with all reserve, I feel that, in a limited number of patients with senile cataract, such as those of great age with severe cough, or a marasmic constitution; or where suppuration or hemorrhage has followed extraction in one eye; or where previous examination of the patient before the lens has become opaque, has shown very fluid vitreous humour, and some others, recourse may be had to this operation, now outside the pale.

Should other surgeons agree with me that art ought to accomplish equally well what accident effects so rudely, and nature spontaneously, I think they will find my plan of operating the most desirable, as it will decide in such cases whether depression is advisable; for should the vitreous humour be fluid, on detaching the suspensory ligament, except at its lower part, the lens will fall away from the pupil, when this movement may be continued by the aid of the instrument downwards and backwards to the fundus, where it will remain partially fixed by the remains of the ligament; but, should the lens retain its position, the operation of extracting it in its capsule will probably be the better one.

Von Graefe has stated that a lens, perfectly freed from its detachment, is less likely to set up glaucomatous symptoms than one partially detached; but, in the plan proposed, the lens is practically free, although still adherent at one part.—*British Medical Journal, Dec.* 30, 1882, *p.* 1292.

74.—ON THE EXTRACTION OF SENILE CATARACT IN ITS CAPSULE.

By Edwyn Andrew, M.D., M.S., F.R.C.S., Shrewsbury.

Without dwelling on the minor points, I will lay down the various means which I have used at the Shrewsbury Eye Hospital, with the design of rendering the operation more easy of performance, and more certain of being successful.

Previous Examination.—The eye is tested, some days before operating, with regard to its sensitiveness to atropine and eserine, so as to ascertain the weakest solution of atropine which will produce the greatest dilatation of the pupil; and, secondly, the percentage of eserine which will overcome, if possible, that dilatation; as we thus learn the dilatability of the pupil, whether the lens will pass through it without bruising, whether an iridectomy would be requisite, what power we possess to overcome a tendency to prolapse, or, what is more frequent, dislocation of the pupil. The lens is examined with special reference to size of corneal opening required for its easy delivery, whether large or small, flat or arched, whether con-

taining a large or small nucleus, surrounded by much or little, soft, or semi-hard cortical matter, *e.g.*, a milky white lens with a small nucleus, surrounded by fluid cortical matter, will require a much smaller corneal opening than the opposite condition.

The state of tension of eyeball and of the vessels in other parts of the body with regard to elasticity or hardness, is carefully studied; for, if ossified vessels exist with increased tension, this operation is not advisable, lest the large corneal opening required, and removal of the posterior part of the capsule should induce rupture of the choroidal vessels, frequently weak under such conditions.

The size of the anterior chamber, the diseased or healthy condition of the other structures of the eye, the congestion or otherwise of the conjunctival vessels, the patency or obstruction of the lachrymal apparatus—all are noticed with regard to the future steps of the operation.

Previous Preparation.—The canaliculus is slit up if there be any lachrymal obstruction; the eyes are bandaged for some days, so that the patient may become accustomed to the feeling; the morning and evening temperatures are noted; the night before operation, atropine of sufficient strength is instilled to produce moderate dilatation next day. On the morning of the day, a very light breakfast is given, such as may be digested before the operation at midday.

Anæsthetics, generally ether, are almost always employed; and, should a large corneal opening be made, they are most desirable, if not essential. Their effect should be complete; there should be no reflex action on the eye being touched; if difficult to be accomplished, they should be aided by a hypodermic injection. In those rare cases, as in intemperate patients, where still ineffective, and especially when ether being used is accompanied by blueness of face and congestion of the superficial veins, and probably engorgement of the choroidal vessels, chloroform should take the place of ether, or the operation should be modified, or, better still, another chosen.

To entirely prevent the action of the muscles is to remove one great cause of the loss of vitreous. As a strenuous advocate of anæsthetics for many years, my experience is, that loss of appetite in old people has been their chief evil, sickness being of little importance if the stomach has been previously kept empty, and if moderate pressure be kept up on the eye by a bandage; whilst their advantages in this operation are so great compared with their disadvantages, that their use is strongly urged.

Antiseptics are not employed. Many operators, especially Pagenstecher, say they render the danger of an eye operation,

especially this one, very much less, and in particular, remedy the disaster of a large loss of vitreous humour. On the other hand, De Wecker finds the results are no better with than without. In my own practice, perhaps from imperfect application, or a difficulty in carrying it out, they have been found objectionable rather than otherwise. For years I have carried out a plan which I can recommend to all, viz., to have a small tin of water always kept boiling by a gas-jet, into which every instrument is dipped before use; this, if it do not destroy every germ, must at least, I should think, check their ardour, and has this advantage, if no other, of keeping my instruments in beautiful condition.

Specula.—None are used, owing to the indirect pressure on the eyeball mentioned above. In lieu of these, at first the upper lid was raised by a retractor held by an assistant, but it was found to be in the way, and easily slipped; next, a *serre-fine*, to which a thread was attached to seize the skin of the lid, but it too often tore the skin away, and became detached; but now, and for a considerable time past, a carbolised ligature has been introduced through the skin of the upper lid, which, without inflicting any appreciable injury, forms a most perfect speculum (if I may so call it) for the use either of the operator or of his assistant; it is entirely out of the way, it causes no pressure, and it allows the eye to be instantly opened or shut as frequently as may be desired. The same may be used on both lids, but for the lower it is rarely required.

Section of Cornea.—This has commonly been made in or near the corneo-sclerotic junction, the extent of the incision generally varying from five-twelfths to one-third of the circumference of the cornea, according to the condition and size of the lens; when hard, a large corneal opening being required, and *vice versâ*. If in doubt, the larger section has been always made. All these corneal sections have been made with a modified Gräfe's or Beer's knife, even on the temporal side, in preference to Macnamara's triangular knife; this being accomplished by using a double forceps rotating the ball, and thus allowing the knife to be used. When, however, with a widely dilated pupil, the temporal section has been determined on, the triangular knife has been used so as to prevent the almost certain injury by the ordinary knife. The upper section is generally selected if the eyeball be in good condition, if there be no intention of using the scoop, or if an iridectomy is to be performed.

The lower section is made, if the eyeball be diseased, especially if the tension be lowered; the temporal or the outer and lower section if the scoop be used, as in either of these positions the instrument can be most easily passed under the lens without rupturing the vitreous body to any great extent.

The corneal section of Liebreich and Lebrun also readily allows the removal of the lens in its capsule, with probably less tendency to escape of vitreous humour, but with the disadvantage common to all such openings, the great liability to anterior synæchia. A nasal section has been made with a bent triangular knife, but it possesses no advantages.

Iridectomy, which diminishes the conservative aim of this operation, is only practised when there is considerable tension, posterior synechia, a very shallow anterior chamber, or when the pupil is not dilatable by atropine, or only slightly so. In the first four of these conditions, a moderate or a large portion of the iris is removed. When slightly dilatable, a minute portion is taken away, or a simple incision is made through the iris by a capsular or other scissors. Under these circumstances, iridectomy is generally performed at the time of extraction; but when the eye is much diseased, it should be done a few weeks previously.

Extraction.—To render the use of the scoop the exception rather than the rule, the suspensory ligament of the lens has been detached by a new method. With a piece of stiff wire, about one inch in length, mounted on a handle, having the point smooth and rounded, and a line to half a line of its extremity bent at a right angle, an instrument has been formed, to be used in the following manner, either before or after the section. If before, a minute opening is made in the corner close to its margin by a small bent broad needle; the instrument is passed into the anterior chamber sideways over the lens and under the iris until it reaches its distal margin; the point is now turned downwards, the ligament pierced with ease, and following the edge of the lens as a guide, it may be torn to any extent the operator may desire by rotating the ball. If, after the section, the instrument is introduced through any part of the opening found most convenient, and whichever course is followed, being under the iris, that structure is scarcely touched. The endeavour is to tear the ligament with the slightest injury to the vitreous body; and, to accomplish this in the manner above mentioned, the bent portion is made of different lengths, in order to adapt itself to an arched or a flat cataract. The wire is also fixed in a light handle, such as cedar-wood, so that the slightest resistance at the point is communicated to the fingers. The lens being thus quite freed from its attachments—for the slight adhesion to the hyaloid fossa is of no importance—and the patient perfectly quiescent, the lens, by slight pressure with a tortoiseshell curette, may be generally eased through the corneal opening without the aid of any extracting instrument.

Pressure failing, should the vitreous body be of moderate

consistence, as shown by its projecting with a glass bead-like appearance, instead of the ordinary scoop, another has been used which I had made some years ago, with the shaft bent backwards near the end, so that the loop may be passed in front of the lens to catch it at its distal margin, and to draw it forwards, raising the cornea at the same time without the scoop touching either iris or vitreous body. Should the vitreous humour be very fluid, the ordinary scoop passed under would be preferable. In other cases, where moderate pressure causes the lens to project without escaping, elongated scoops formed like the blades of a diminutive midwifery forceps, of different curves, have been applied to the edges of the lens to assist its exit, with great advantage.

Again, in order to lessen, especially in diseased eyes, the evil of a large corneal opening, and the diminution of the natural internal support of the ball, modifications of this operation have been and are being tried, but not sufficiently often to give the results that weight required in an assembly like this; I shall therefore content myself by mentioning them.

As the lens becomes slightly wider, when pressed between the scoop and cornea on its passage outwards, thereby necessitating a rather wider corneal opening, and as pressure on the edges of the lens can so compress the cortical matter, without rupturing the capsule, as to render the lens oval instead of round, so as to be capable of passing through a smaller corneal opening, straight and curved forceps with minute scoops at their ends have been constructed to effect this purpose.

The suspensory ligament has been detached, and the lens left loose in the eye for several weeks before extraction. As the eye has been found to suffer no appreciable injury from the freed lens within it, the detaching instrument has been used diagnostically to indicate the best means of removing the separated lens, the size of the corneal opening required, and the advisability or not of an iridectomy; and again, in exceptional cases, the desirability of depressing, as has been explained more fully in my paper on Dislocation of the Lens (*vide* Article 73). A small corneal section has been made, the suspensory ligament has been detached, except adjacent to the section; a portion of iris has been removed, the edge of the capsule has been divided after the manner of Knapp, the cortical matter and nucleus have been pressed or scooped out, and the empty capsule previously detached, in great part has been removed at the same time or at a later date.

The lens, with its capsule entire, being removed by one or other of these methods, the upper lid is slightly rubbed and then raised, when, if the pupil be round, central, and of normal size, no application is used; if dilated, eserine is applied; and should

the pupil show irregularity, eserine is continued for several days; a strip of isinglass plaster is first put over the lid, followed by a piece of lint dipped in carbolised glycerine, and over that a pad of cotton-wool; the whole is kept in position by as much pressure as is agreeable to the patient by a carbolised gauze bandage. If there be pain, which is rare, it occurs generally the first night, when relief is usually obtained by applying a leech, followed by an opiate pill or a hypodermic injection. The patient progressing, no treatment is required except the daily reapplication of the dressings for his comfort; he is allowed to get up the third day, and generally commences using his eye about the tenth or twelfth. If the various steps of the operation have been performed satisfactorily, a successful result is almost certain, provided the retina retains sufficient visual power; but, as in other operations, difficulties arise, which at times, with every care, mar these results.

The anæsthetic may act imperfectly, causing struggling or muscular spasm; or, when an anæsthetic is not used, the patient may have too little self-control, causing unsteadiness; in either case inducing too great loss of vitreous humour, or bruising the structures from the consequent difficulty of using the instruments with the required delicacy. In such circumstances, dislocation of the pupil is the least evil; as, although it disfigures the eye, it still allows of good vision. Pushing forward of the whole iris against the cornea is more serious, as it shows disorganisation of the vitreous structure, necessarily with diminution of sight, and often ending in total blindness. Prolapse of the iris is rare, primary iritis still more rare; but, at a later date, when much vitreous humour has been lost, permanent hyalitis, with secondary iritis, too often destroys vision by exudation of lymph. Suppuration I have never seen, unless, with other injuries to the eye, cortical capsular matter have been left behind.

Putting aside senile cataracts of old standing, with increased tension and diminished anterior chamber, this operation is suitable to almost all other cases of senile cataract, especially when joined with synechia, myopia, irido-choroiditis, and other conditions, where it is particularly desirable to avoid all irritation after extraction. I look forward to the time when, rendered safer by experience, it will be employed to remove cataract at a much earlier date, before the vitreous body and other structures of the eye have been weakened; so that, instead of allowing a patient to grope about waiting for his cataract to mature, we shall feel justified in advising its removal as soon as it becomes a decided inconvenience.

In concluding these remarks, I claim for my operation that it lessens external pressure, and prevents internal bruising;

that it requires no specula, but provides an easy and safe mode of opening the lids; that it does not mutilate the iris; that the use of the scoop is the exception; that it seldom, if ever, ruptures the capsule, the preservation of which is the chief safeguard in all such operations; and that this mode of operating gives visual results equal, if not superior, to any other mode of extraction, with the impossibility of a secondary cataract.—*British Medical Journal, Jan.* 13, 1883, *p.* 41.

75.—ON THE EXTRACTION OF CATARACT BY A SHALLOW LOWER FLAP.—SERIES OF 121 OPERATIONS.

By Simeon Snell, Ophthalmic Surgeon to Sheffield General Infirmary, and to the School for the Blind.

Before proceeding to describe the operation performed in these cases, I would say that only in three instances (one further on excluded from consideration) has an anæsthetic been administered. In each of these, ether was given, though in one, owing to the excitement produced, chloroform was substituted. I have always avoided the use of anæsthetics, because the comparative brevity of the operation not only generally renders their employment unnecessary; but I fancy the dangers resulting from the movements of the patient less, instead of greater, than those from the sickness and retching after the administration of chloroform or ether, to say nothing of the general upsetting which, I think, must sometimes exert a deleterious influence. Passing reference may be made to the recently recorded statistics of Hasket Derby, both with and without anæsthesia, and to the superior results achieved under the latter.

As to the operation, no speculum or fixation-forceps is employed. The eyeball is steadied only by the operator's fingers in the following manner. The patient, lying flat on his back, either on the operating table, or on a bed, the surgeon stands behind, and, with the fore and middle fingers of the left or right hand, according as it is the right or the left eye which is to be operated upon, gently draws upwards the upper lid. This being done, the middle finger is allowed to descend somewhat, until by gentle pressure on the inner side of the sclerotic, it steadies the globe. At the same time, an assistant draws away the lower lid, and fixes it against the malar bone. An ordinary Gräfe's knife is now used; sometimes, however, I employ one narrower and shorter, the operator using the left hand for the left eye, and the right for the right eye if he be ambidextrous, and the puncture made in the sclero-corneal junction just below the centre of the pupil. The knife is entered flat, or nearly so, and the counter-puncture having

been completed, it is carried downwards for a little distance, and then the edge of the knife is turned directly forwards, and the section finished. The summit of the flap will thus lie about the middle of, or a trifle below, the centre of the space between the pupillary edge and the lower corneal border. The capsule is freely torn with a bent cystotome, and the lids freed from all moisture by portions of lint. The lens now finds a ready exit by means of pressure on the upper lid by the fingers, and by the same plan on the lower, but, generally speaking, with the vulcanite curette. Any remaining cortex is got rid of by gently rubbing the lid over the cornea, and what is well known as the "lid manœuvre," and the operation now finished, over each eye is placed a fold of lint, that over the one which has undergone extraction is moistened in cold water, and desired to be kept so for the next few days, and then a thin bandage completes the dressing. The eye is, usually speaking, left undisturbed until the fourth day. At this time, a peep is made to see that everything is favourable; and, if so, the patient is allowed to sit up, for a little, on the fifth or sixth day.

With respect to the performance of iridectomy, it will be presently shown that, in less than half the cases, a portion of iris was removed. The rule I follow is simply this. Should the iris show signs of clinging about the wound after the escape of the lens, and not readily receding, a small piece is cut off; or, indeed, sometimes I excise a portion before the removal of the cataract, or the rupturing of its capsule. If, for various reasons, I fancy there be a little more risk in the operation in a given case, an iridectomy is performed. Generally speaking, it is almost a matter of indifference as to whether a segment of iris be removed or not, as I am not certain that cases do better for the iridectomy. For cosmetic reasons, one would avoid it as much as possible.

Sponges, however clean, and however antisepticised, I have, for some time, entirely discarded, and in their places used portions of lint, either dry or moist. In a small number of cases, I adopted, with slight modifications, the antiseptic plan suggested by Alfred Gräfe, and which I recorded in the Journal (1880, vol. i., p. 241). The eye in these cases was well washed with a 2 per cent. solution of carbolic acid, and was then, until the time of the operation, covered over with a sponge wetted in the same solution. The instruments were dipped in alcohol and dried on lint before their use; only carbolised sponges were employed during the operation, and a pad of lint, soaked in a 4 per cent. solution of boracic acid applied subsequently. The carbolic acid caused such irritation and swelling of the conjunctiva, with thick gummy discharge, that I

was soon led to abandon its use, and in one instance substituted thymol for it. The convalescence of these cases, treated with such care, appeared to me somewhat prolonged; and I saw no reason to relinquish a procedure with which I was in no way dissatisfied, and to which I was accustomed, and I therefore returned to it. For some time, however, I have dipped the instruments into alcohol before use, and, still more recently, have adopted also a very modified antiseptic plan in substituting for the moistened lint pad one of salicylic silk, suggested by my friend Mr. McGill of Leeds for general surgical use, and which I have found very serviceable in various ocular and aural cases. Its lightness and softness makes it a convenient pad, and thus far I am pleased with its use. The importance of absolute cleanliness in the instruments, the nurse, the patient, and his surroundings, are points which are daily testified to, as of the highest importance towards attaining a high percentage of successes in cataract-extractions.

As to the advantages and merits attached to the operation here described, it would, in the first place, seem that the exit of the lens is more ready downwards than through an aperture situated above. This partly depends, no doubt, on the ease with which the lower lid is kept free from the wound, contrasting thus with the greater difficulty, except with the use of speculum, that the upper lid is retracted. The operation I have mentioned is, moreover, soon completed, and not fraught with any great discomfort to the patient, to which end the absence of speculum and forceps lends a conspicuous aid. There is thus also no necessity for an anæsthetic.

It has the merit, besides, of being easier of performance than many other modes of procedure, and, the flap being the section of a much larger circle than the circumference of the cornea, gives more space for the exit of the lens, than one made more peripherally would do. The section is confined entirely to the cornea, and, as has just been stated, yields an aperture of sufficient size without encroaching on the dangerous ciliary region. Escape of vitreous humour is only of rare occurrence; but this will be referred to again in the statistical analysis.

Various operators have during recent years advocated the removal of cataract downwards, with shallow flaps. Liebreich and Bader have both suggested and practised it. The operation I have described differs from the former of these, not to go into other points, in which the puncture and counter-puncture are made in the sclerotic, beyond the sclero-corneal junction. It resembles Bader's, in that the section is strictly corneal, but differs in the formation of the flap. Quite recently, Mr. Lloyd Owen, of Birmingham, has described an operation with the lower section, and by a small angular flap. This would seem more to resemble

the operation I have practised for several years. The summit of the flap is, however, more peripheral, I fancy, than in the one I have described. Among the disadvantages said to belong to extraction, in the manner just mentioned, is the adhesion of iris to the wound. This undoubtedly does occur sometimes; but not so often, however, in my experience, as has been alleged, and probably not more frequently than when rags of iris were left entangled in the corners of a Gräfe section.

The importance of getting rid of the entire cataract is, of course, of the first moment, and any particles of cortex are generally readily removed by means of the "lid manœuvre." The introduction of instruments is strictly avoided; and, generally speaking, the only ones introduced during the operation are the knife and the cystotome; and when a portion of iris is removed, it is seized by forceps as it lies prolapsed at the wound. Should the lens for any reason not readily escape, and it be necessary to facilitate its removal, I have found Taylor's vectis of the greatest value; and I fancy an advantage will be noticed in having the instrument made entirely of platinum—the loop as well as the stem. By this means, it is not only possible to bend it into any position desired; but it is readily cleaned by passing it through the flame of a spirit-lamp.—*British Medical Journal, Jan.* 13, 1883, *p.* 44.

76.—ON CERTAIN MODIFICATIONS OF GRAEFE'S LINEAR OPERATION FOR EXTRACTION OF CATARACT.

By George Cowell, F.R.C.S., Senior Surgeon, Westminster, and Royal Westminster Ophthalmic Hospitals.

I have, from each new method, reverted with satisfaction to von Gräfe's modified linear extraction; but I was early induced to introduce certain minor modifications of my own, or, rather, I would prefer to call them attentions to minor, yet important, details, for every surgeon knows that it is on these that the success of all operations, in a large measure, depends. These modifications, if such they may be called, may not suit everyone's mode of operating; but a careful attention to them will minimise dangers, and, in my own hands, they have been attended with a very considerable success, as I shall show by giving an analysis of 100 consecutive cases. But, first, let me describe the points to which I attach importance, and the modifications that I have followed.

1. I always have the pupil well dilated with atropine on the morning of the operation.

2. I always give an anæsthetic, unless it be contraindicated, or unless I can be sure of the complete quietude of my patient. Only six per cent. have been operated on without.

3. The section is corneal, but sufficiently peripheral to be sub-conjunctival; the puncture and counter-puncture, which are a little lower than in von Gräfe's operation, alone being just outside, or in the sclero-corneal junction, according as there is reason to believe that there is a large or a small nucleus.

4. The section is made with extreme deliberation, with scarcely more pressure than the weight of the knife; and, in making the middle of the section, the edge of the blade is not turned much forwards, the desire being that the middle of the section should be only just within the margin of the cornea. It is important that the knife be sharp, in order that no force be used in making the section, thereby avoiding all risk of disturbing the coats and contents of the eye.

5. Sometimes, but not always, after the section has been made, the end of the section answering to the counter-puncture is just touched by the end of the blade of von Gräfe's knife, so as to make the section of the internal surface of the cornea correspond with the section of the outer surface, as is already the case at the point of puncture. This is important only when the section is somewhat small, or the nucleus large.

6. A period of rest of at least one minute is taken after each step of the operation, to give the eye time to adapt itself to the new conditions of circulation and tension.

7. A free crucial incision is made in the anterior capsule.

8. A gentle coaxing out of the nucleus and all the soft lens matter is made with the smooth back of the curette, taking care that the surface of the cornea is moist before passing the curette over it, avoiding, at the same time, all force, and using the hook at once if the nucleus does not readily escape.

9. A careful tucking-in is made with the end of the curette, of any portion of iris or uvea that may be occupying the angles of the section. A neglect of this precaution sometimes postpones the healing of the wound, and leads to the formation of a cystoid cicatrix, which may, at some future time, be a cause of irritation in the eye. It is also important carefully to adjust the conjunctival flap over the wound.

I use the same instruments as were recommended by von Gräfe, except that I prefer the iris-forceps curved at the points, instead of straight, and the iris-scissors made in the form of forceps, as they are then as convenient for use with the left hand as with the right. As a result of an attention to these points, I claim:

1. An immunity from rupture of the hyaloid, or what does duty as a hyaloid, and, therefore, from loss of vitreous humour, except when the latter is very unhealthy, about five per cent.;

2. Diminished frequency of the occurrence of iritis and other inflammatory conditions;

3. A more rapid convalescence, twelve to twenty days; average fifteen days;

4. An increased proportion of cases in which the patient is able to read "brilliant" type.

The after-treatment is that recommended by von Gräfe. It is rarely necessary that the patient should be kept in bed beyond the third day, or confined to the house beyond the twelfth or fourteenth. I most rigidly abstain from looking at the eye before the fourth day, and often wait until the sixth or seventh. I have for many years given up the practice of keeping my cataract patients in a dark room. A curtain, or screen, placed between the patient's head and the window or light is sufficient, the eyes, of course, being tied up with pads and bandage. I need hardly say that the healing of the wound takes place more readily, and that the health of the patient is better maintained if, whilst the eyes are carefully shielded from the light by the proper pads and bandage, and a light shade of good size, the body be placed under the usual conditions of light and air.—*British Medical Journal, Jan.* 13, 1883, *p.* 49.

77.—ON A NEW METHOD OF CATARACT EXTRACTION WITHOUT EXCISION OF THE IRIS.

By Dr. H. Galezowski, Paris.

The operation for cataract has always occupied the attention of surgeons, and many methods have been devised for the extraction of opaque lenses. One of the best of these methods was that of Daviel, by flap extraction, which gave numerous excellent results. Unfortunately, however, it not unfrequently happened that the most severe suppurative inflammation supervened even when the operation had been most skilfully performed, necrosis of the cornea and panophthalmitis destroying the eye. In presence of such facts it came to be asked whether a very large corneal flap was not the cause of the suppuration, and whether it would not be remedied by modifying the form of the incision. It was on this account that the old method by flap extraction was replaced by the method of extraction by peripheric linear incision. Our eminent London colleague Critchett, whose loss is regretted by all, brought forward an operation which bears his name, and which consisted in a peripheric linear incision of the cornea, iridectomy, and extraction of the lens with a large curette. After this came the method of von Graefe, or modified linear extraction, which, as is well known, was very soon tried in every country in the world and accepted as being the best, for it did not give rise to suppurative complications. As far as I am personally concerned,

I had adopted this last method, and practised it with various modifications, till the last four months. For a long time I obtained satisfactory results, but occasionally I had to record cases of panophthalmitis in patients in whom there was not the slightest reason to anticipate any complication. I have for the last two years employed the most rigorous antiseptic precautions, but this has made no difference. Last year I had five cases of panophthalmitis in ninety-three operations, and another Parisian oculist has recorded seven cases of panophthalmitis in 150 operations, and in several other records of statistics similar data are to be found. What conclusion should one draw from this? That iridectomy as well as the linear peripheric operation do not prevent subsequent inflammatory mischief. This conviction seemed all the more plausible to me when, at the beginning of the present year, I saw a panophthalmitis appear in one of my town patients who was in an excellent state of health, and who had undergone the operation of iridectomy two years previously with a view of facilitating the subsequent operation. I extracted his cataract under the most favourable conditions; the operation went off perfectly well, and strict antiseptic precautions were observed. What then was my astonishment when, within the first twenty-four hours, the symptoms of suppuration of the cornea with consecutive iritis appeared. It is difficult to find an explanation of this occurring notwithstanding the iridectomy, but it became evident to me that iridectomy did not prevent such complications. I then formed the idea of returning to the old French method of flap extraction without iridectomy, but I took care to make a different kind of flap. The results I have obtained seem to me so satisfactory that I have thought it useful to record them, and to show how unjust it was to attribute to the iris all the accidents which supervene after an extraction of the lens.

In making an analysis of the 959 cases of cataract extraction that I have performed since 1874, and of which I have preserved the records, I came to the following important conclusions:—1. That the excision of a portion of the iris does not in any way prevent inflammatory complications subsequent to the extraction of the lens. 2. That it is often the cause of secondary cataract. 3. That sclerotic or sclero-corneal incisions are far more prone to cause subsequent complications than corneal incisions, and that they do not prevent subsequent severe inflammation of the cornea and iris. In presence therefore of failures in my own practice, which could not in any way be foreseen, and taking into consideration the considerable number of failures recorded by De Wecker, Abadie, and others, I determined to modify the operative procedure and to bring it back as near as possible to the old French method or method of Daviel. The modification

I employ refers to the form of the corneal incision; thus instead of having a large flap whose periphery is at all points 2 mm. distant from the sclerotic border, I make my puncture and counter-puncture in the cornea, just where it joins the sclerotic, but I make the summit of my flap 2 mm. distant from the superior sclerotic border. The incision is therefore entirely corneal. By this procedure the resulting flap is broader and not so high as in the old method of Daviel, and the incision is elliptical instead of being spherical, and in consequence coaptation is easier and cicatrisation more rapid. In giving this form to the incision a gain of 2 mm. on each side is obtained, and this allows even the most voluminous cataract to emerge without difficulty. The pupil dilates under the pressure of the lens and allows its free passage and then returns of itself to its former position, or is replaced by the aid of a fine silver stylet. Up to the present time I have performed fifty-six operations by this method, and I have not had any serious subsequent complications.

The following are the details of the operation I now almost exclusively perform:—The patient is placed upon an operating table or bed, and his eyelids separated by my articulated spring speculum. I then seize the conjunctiva just below the cornea with a fixation forceps, and gently draw the eye slightly downwards. I then puncture the transparent cornea just at its junction with the sclerotic with a very narrow-bladed von Graefe's knife, and at a point 3 mm. above the horizontal diameter. Pushing the point of the knife downwards to the border of the pupil, which has previously been dilated, I press on the capsule, incising it from below upwards, then, slightly withdrawing the knife, I push it into the transverse diameter of the capsule, which I then incise in its entire breadth, after which I make the counter-puncture at the opposite corneal border at a point also 3 mm. above the horizontal diameter. Then inclining the edge of the knife slightly forwards I cut the corneal flap in such a manner that its summit shall be 2 mm. distant from the superior sclerotic margin. This first step in the operation terminated, I remove the fixation forceps and spring speculum, and let the patient rest for a few seconds. I then continue the operation by elevating thoroughly the upper lid with my little finger, which, as well as the curette, presses upon the sclerotic border of the incision whilst the patient looks strongly downwards. With the thumb of my left hand I gently press upon the inferior border of the cornea, and then the lens is seen first to engage in the iris, then in the incision, and, finally, to emerge from the eye.

It frequently occurs that after the lens has emerged the iris returns of itself, and the edges of the incision come into appo-

sition. In other cases the iris protrudes, and must then be replaced in the anterior chamber by a fine silver stylet. If any of the cortical substance of the cataract remains in the pupil, it is removed by a curette, which is introduced several times into the anterior chamber or the pupil. The operation being finished, and the eye being open, I turn the carbolic spray on to the eye, and dress with carbolised cotton-wool, and bandage both eyes. As a rule the coaptation of the wound takes place in twenty-four or forty-eight hours. In one case it did not take place for six days. On three occasions there occurred slight attacks of iritis on the seventh, eighth, or tenth day, which easily yielded to antiphlogistic treatment, and on one occasion severe iritis appeared within the first twenty-four hours, but yielded to douches by the carbolised spray and to the application of leeches.

I have had two cases of hernia of the iris, both of which occurred on the fifth or sixth day, and in both cases I had to excise the protruded portion. The final results after extraction by this method are relatively superior to those I obtained with iridectomy. This I have had evidence of in two of my patients who underwent the operation for cataract, one twelve months and the other three years ago. In one of these cases one eye was operated on with iridectomy, and the other without. The visual keenness obtained in eyes operated on without iridectomy is superior by one-third or one-fourth to that obtained when iridectomy is performed. It will be easily understood that an eye with a round pupil is much better protected against excessive light, is less dazzled, and the dispersion of luminous rays is much less. Since my recent experience has demonstrated to me the considerable advantages of cataract extraction without iridectomy, I never touch the iris except in the following exceptional cases:—1. When during the incision of the cornea the iris bulges forward in front of the knife and is wounded. 2. In traumatic cataracts complicated by posterior synechiæ. 3. In cases where the lens covered by the iris does not engage in the incision. 4. I excise the iris when I perceive that it has been torn during the passage of a hard and voluminous lens.—*Lancet, Jan.* 20, 1883, *p.* 92.

78.—ON THE USE OF ESERINE AS A PRELIMINARY TO EXTRACTION IN CASES OF CATARACT.

By Charles Bell Taylor, M.D., F.R.C.S., Surgeon to the Nottingham and Midland Eye Infirmary.

In selecting an operation for cataract, the surgeon will always have two ends in view; first, to adopt such a method as will secure the smallest percentage of positive failures, and second,

one that will give the most brilliant results, both as regards appearance and vision.

Unfortunately, these two ends are not always compatible. What may be done in the way of cataract operation, I shall illustrate by two patients who are subjects of double extraction with central and movable pupils, who are kind enough to attend here to-day; as also by another patient who has had both eyes operated on in the manner I am about to describe.

No one—if you will pardon a slight digression—who has had experience in ophthalmic surgery, can have failed to appreciate the advantage of a background of iris when extracting small chips of steel or other foreign bodies from the surface of the cornea. Unless the eye can be turned in such a direction as to obtain this advantage, there is apt to be a want of precision in our proceedings; and, when the foreign body is situated exactly opposite the black expanse of the pupil, it may be either overlooked altogether, or attempts at extraction may be attended by unnecessary denudation of the cornea. A drop of eserine obviates this inconvenience, and the foreign body, thrown into strong relief, is readily removed without doing violence to any of the tissues not actually involved.

Now, if a background of iris is a convenience when operating for the removal of foreign bodies from the surface of the cornea, it is equally advantageous when operating for extraction of cataract; while a deep anterior chamber, and a pupil so contracted as to reduce the chances of subsequent prolapse to a minimum, are so manifestly in favour of the operator, that I cannot doubt that the instillation of eserine will speedily become generally adopted as the first step in the operation of extraction for cataract, as also in some cases where it is necessary to form an artificial pupil, or to remove foreign bodies from the cornea. I generally apply the solution half-an-hour or twenty minutes prior to commencing the operation; which, premising that I always secure complete control of the eyeball by forceps that pierce the sclerotic, may be accomplished, according to the varying exigencies of each individual case, by one or other of the following methods.

First, I use an extremely narrow-bladed knife; enter it with the edge directed upwards in the corneo-sclerotic junction on a line with the pupil; bring the knife out exactly opposite, and then cut upwards with a gentle sawing motion until close to the corneo-sclerotic junction of the upper third of the cornea, when the edge of the blade is turned forwards and the section completed. I then excise a few fibres of the upper segment of the sphincter of the iris, lacerate the capsule, and extract in the usual way. It is only in very exceptional cases that I find it necessary to administer an anæsthetic; and, in a majority of

patients, I am in the habit of manipulating the knife so as to get a portion of iris across the edges, completing the iridectomy and section at the same time.

The only delicate portion of the operation now remaining is the laceration of the capsule, which, when possible, I dispense with, and extract the lens and capsule entire by careful manipulation on the surface of the cornea. The operation may be simplified by a preliminary iridectomy, which I accomplish by a small incision midway between the upper edge of the pupil and the corneo-sclerotic junction, lacerating the capsule at the same time. If this be done half-an-hour before the extraction, the aqueous humour is resecrated, and the only risky part of the operation is completed when the eyeball is thoroughly under control. Or, instead of completing the incision, I leave a bridge in the upper third of the cornea, make an incision in front of this bridge, perform a slight iridectomy and lacerate the capsule, dividing the bridge as the last stage in the operation; or a bridge may be left at the angle of the wound, which is subsequently divided with scissors. If it be desirable to extract without iridectomy, I make an incision with the pupil under eserine, and extract when the contraction caused by eserine has been overcome by atropine, which takes place in about half-an-hour.

The Use of Eserine as a Preliminary to Extraction.—Iridectomy purposely effected with the knife, or half-an-hour before the removal of the lens or under the protection of a bridge which is subsequently divided, is, so far as I know, a novel feature in the operation of extraction for cataract. In my hands eserine has proved specially useful; and I have here a patient in whom the iridectomy was effected in the left eye with the knife in the right eye half-an hour before extraction. The two other cases of double extraction in which the iris is untouched demonstrate the brilliant results which are obtained by preserving a central and movable pupil; and, had it been practicable, I could have introduced several patients illustrating what I conceive to be the advantage of one or other of these modes of procedure in suitably selected cases.—*British Medical Journal, Dec.* 30, 1882, *p.* 1293.

79.—NEW CATARACT OPERATION.—DR. TAYLOR'S KNIVES.

By Dr. Charles Bell Taylor, F.R.C.S.E., Nottingham.

I have frequently performed an operation similar to that described by Prof. Galezowski—extracting by a small corneal flap, and preserving the pupil entire. I introduced some patients who had been operated on in this way to the Clinical Society of London, in 1876, and again to the members of the

British Medical Association, at Worcester, last year. The results are undoubtedly most brilliant; but with a narrow-bladed knife, such as Galezowski describes, the iris is certain to fold over the edge of the blade as it passes across the anterior chamber, and it is always difficult, and sometimes impossible, to complete the incision without mutilating the pupil. In order to obviate this inconvenience, I have been in the habit of instilling a strong solution of eserine half an hour before commencing the operation, and making the incision with knives such as I have figured. These instruments possess all the advantages of Prof. Graefe's narrow-bladed knife. Their convex form prevents the premature escape of the aqueous humour, and it is consequently easy to prevent prolapse of the iris, which has always been considered the bugbear of extraction. The bent blade enables the surgeon to use the right hand when operating upon the left eye. The knives have been made for me by Weiss, and are represented the exact size. As I very seldom use an anæsthetic when extracting for cataract, and, during the winter months, usually employ my refracting globe with artificial light, I find no difficulty in deferring the completion of the operation until the pupil is again dilated with atropine, which takes place in about half an hour. I may mention that in one hundred patients operated upon within the last few months, with and without iridectomy, I have had only two cases of suppuration of the globe. Both of these were double extractions, at one sitting, in very feeble persons, advanced in life. I am thankful to add that in each one eye was saved, and sight restored.—*Lancet, April* 7, *p.* 613.

80.—ON THE TRANSPLANTATION OF SKIN *EN MASSE* IN ECTROPION AND OTHER DEFORMITIES.

By Charles Bell Taylor. M.D., F.R.C.S., Surgeon to the Nottingham and Midland Eye Infirmary.

The transplantation of skin from one part of the body to another, and from one person to another, without pedicle, is a subject of so much importance that I cannot doubt that a brief record of cases in which this operation has been performed by myself will be acceptable. Although this method of treating deformities has not attracted much attention until recently, it was practised in 1818 by C. F. Graefe, who transplanted a piece of skin the size of a fourpenny-piece; and Mr. Lawson, the well-known ophthalmic surgeon, succeeded in 1870 in establishing a graft the size of a shilling; in the same year Leon Lefort, one of the surgeons to the Hotel Dieu, demonstrated that it was possible to transplant pieces of cuticle without pedicle, and the subject has since been so exhaustively treated by Dr. Wolfe, of Glasgow, that the operation is now very generally associated with his name. Both Lefort and Wolfe insisted upon the absolute necessity of scraping the under surface of the flap until every shred of subcutaneous tissue was removed, and Abadie and others who have practised the operation insist upon the necessity of a like precaution. My own experience, which is not in accord with the above-named authorities, dates from March 1875, when I was induced to replace a large piece of skin that I had removed from the upper eyelid of a lady, in order to remedy a very marked degree of ptosis. So much skin had been removed that the eye would not close, and I determined to replace it. No precautions whatever were taken, and although the flap had been separated from the body for nearly half an hour, and was apparently quite dead, the wound healed like a cut finger. So remarkable a success made a great impression on my mind, and being unaware at the time of what had been done in this direction by Lefort and others, I came to the conclusion that I had stumbled upon a discovery of some importance, and accordingly brought the case in question, together with a child in whom I had replaced the eyelid by a flap taken from the arm, under the notice of the members of the British Medical Association at the annual meeting when held in Leicester in 1877.

This child, who came from a distance, had been seen by other surgeons, and some attempt had been made to remedy the deformity; but the small portion of the upper lid, which had escaped destruction, was so firmly embedded in the diseased tissue that it was evident that nothing but a most careful

operation with transplantation of new skin, necessarily from a distant region, would suffice, while it was clear that, unless something was done, the eye, constantly open, congested, and perpetually exposed to external irritation, would be seriously damaged. It was impossible to preserve a pedicle while transplanting from a distant region, as the patient was exceptionally timid and irritable. I therefore decided to transplant *en masse*, and selected for the purpose a piece of delicate skin from the inside of the arm. The upper lid was turned completely inside out, and I took advantage of this position to make a raw surface well within the lashes; a similar raw surface was established on the opposing edge of the lower lid; what remained of the eyelid cut free from its deep-seated attachments, and the two raw surfaces brought together and firmly attached by sutures deeply placed in the lids. The upper portion of skin, which was buried under the orbital ridge, was then carefully dissected out, and the raw surface between the two covered with the flap borrowed from the arm. The triangular indentation immediately over the brow was filled up with another piece of skin, and the part saturated with a solution of boracic acid in cotton wool, which was maintained *in situ* by a compress bandage. The lids adhered, thus establishing a permanent anchyloblepharon, and, with but a slight threatening of sloughing at the edge, the transplanted skin became firmly attached in its new situation. I maintained the anchyloblepharon for six months, when, finding that the lid was firmly embedded in its new situation, I divided the attachments between the lids, and dismissed the case with the result depicted in the second photograph.

I may here remark that in establishing an artificial anchyloblepharon in order to treat ectropion, transplant tissue, or for any other purpose, it is most important to maintain the eyelashes intact, and, in order to do so, to make the raw surfaces well within the edges of the eyelids, and to maintain apposition by sutures deeply placed. An examination of the second photograph shows that this end has been fully attained in the case in question.

In the preceding three cases no preparation of the skin to be transplanted, such as scraping the under surface as recommended by Lefort and Wolfe, was adopted; the skin was simply replaced just as it was removed, with the fine reticulated vascular under surface untouched. In the next two cases I adopted the precautions recommended by these gentlemen, and both sloughed in spite of every care. In my sixth case I therefore replaced the cuticle simply as in my first cases, and although the lid (the lower lid), the subject of the operation, was constantly bathed in pus from a suppurating eyeball, the wound

healed like a simple incision, and never gave the slightest trouble from first to last.

I have had other cases since I read a paper on this subject at the Jubilee meeting of the British Medical Association in August last, and am inclined to modify the opinion I then expressed so far as to say that I am disposed to think that, if no peeling of the under surface of the flap is adopted, the percentage of cases of successful transplantation would be greater than I then thought possible. I would also remark that the fine silky tissue of the upper lid, which may always be spared, is best adapted for this purpose, and that I have successfully transplanted it on to the lower surface of the eyeball itself, for the cure of symblepharon, both with and without pedicle.—*Practitioner*, *Dec.* 1882, *p.* 428.

81.—ON THE FUNCTION OF THE MEMBRANA FLACCIDA OF THE TYMPANIC MEMBRANE.

By John M. Crombie, Esq. (*Nature*, Dec. 7, 1882).

Mr. Crombie first points out that the membrana flaccida (Shrapnell's membrane) is loose, and only very slightly attached to the bone, which at this part is smooth (margo tympanicus), but is mainly fastened to the skin of the canal; whilst, on the other hand, the lower portion of the tympanic membrane is firm and transparent, and fixed round its whole circumference to the sulcus tympanicus. After considering the characters of the skin lining the meatus, the author concludes that 'from the membrana flaccida of the membrane, which is easily movable at its margin, there is a piece of movable skin running over a smooth polished surface along the whole upper meatus of the bone, which is here bevelled off, and is immediately continuous with the movable membranous roof of the cartilaginous portion of the external passage. The movable piece of skin serves after its manner the purpose of a tendon, and the muscle which mainly plays upon it is attached to this upper membranous wall, at its point of junction with the osseous meatus.' The muscle in question is the lateral portion of the occipito-frontalis —musculus epicranius temporalis—which is anterior to and smaller than the attollens aurem, which forms the remainder of the lateral portion. On contraction, it raises the membranous roof of the canal upwards and slightly forwards, making the movable patch of skin glide outwards, and so telling upon the membrana flaccida, which even in the adult is almost in a line with the upper wall. The muscle has no isolated voluntary action, but is brought into play when the eyebrows are forcibly raised by the action of the occipito-frontalis. The author states that its effect on the membrana flaccida is distinctly visible

through the speculum when the occipito-frontalis is made to contract.

By bringing the membrana tympani into a more vertical position, the author considers that it causes it to assume a position more favourable for hearing. Mr. Crombie was led to make these observations in endeavouring to trace the connection between a box on the ear and rupture of the membrana tympani, and concludes that a smart blow on the side of the head, too often thoughtlessly administered by schoolmasters and parents in correcting children, may cause sudden spasmodic action of this muscle, and thus, through the mechanism described, rupture of the membrana tympani. The author appears to ignore the usual explanation of this accident by the sudden compression of air in the meatus, which must surely be an important factor. —*Dr. E. Cresswell Baber, London Medical Record, Feb.* 15, *p.* 31.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

82.—ON THE CLINICAL CLASSIFICATION OF BACKWARD DISPLACEMENTS OF THE UTERUS.

By G. Ernest Herman, M.B. and M.R.C.P.Lond., F.R.C.S., Physician to the Royal Maternity Charity, &c.

It is a truism in medicine that for treatment to be successful diagnosis must be accurate. The backward displacements of the uterus, retroversion and retroflexion, are met with sometimes without symptoms, sometimes with symptoms; and in the latter case the symptoms, in different instances, differ widely in kind as well as severity. In some cases the symptoms are removed with striking rapidity and completeness by mechanical treatment, while in others the same treatment entirely fails to relieve. Clearly, therefore, all cases of backward displacement of the uterus are not alike; and before we can treat them with any approach to certainty of result, we must be able to make a diagnosis between those cases which require a particular treatment and those which do not; in other words, they must be classified.

The classification at present most in favour depends upon the shape of the uterus; whether it is bent or not; the sharpness of the bending; the greater or less difficulty of straightening it or keeping it straight. Such a classification of course implies that bending is the important feature of the cases to be classified. I believe that bending of the uterus *per se* is, as a rule, an unimportant condition. I have set forth the reasons which induce me to take that view, so far as dysmenorrhœa is concerned, in papers communicated to the Obstetrical Society of London; and therefore I need not here again defend it. I will only make these assertions: that there are many cases in which symptoms associated with retroflexion will entirely disappear, without any appreciable alteration in the shape of the uterus having taken place; and that there are many others in which mechanical treatment, perfectly successful so far as removing the flexion is concerned, fails to remove the patient's symptoms. Cases such as these are not exceptional, but quite common. Of course there are others in which the reverse is the case. But the existence of the former class of cases is enough to show that a classification based merely on the shape

of the uterus is not an adequate one, not one which helps in the treatment of backward displacements of the uterus.

To make clear the principles upon which I think that distinctions should be drawn between different cases of backward displacement, I would ask attention to the circumstances in which such displacements occur, and the conditions upon which, in my opinion, the symptoms connected with them depend.

The essential abnormal condition which produces these displacements is yielding of the pelvic floor—that is to say, the muscles, fasciæ, fat, and cellular tissue, which form, as Dr. D. B. Hart has pointed out, a compact layer closing the abdominal cavity below. I see no reason for attributing them to an isolated alteration in any single structure; for it has been shown by experiment that different component parts of the pelvic floor may be separately divided without producing descent or displacement of the uterus. When slight yielding of the pelvic floor takes place, one of the first results is that the mobility of the uterus is increased, and that this organ slightly sinks. The greater mobility of the uterus which the loosening of its supports obviously involves allows it to fall either forwards or backwards. If it were rigid, a falling forwards or backwards of its body would be accompanied with a relative elevation of its cervix in the contrary direction, seeing that the uterus is most firmly fixed at about the level of the os internum. Often this is what takes place, and then we have anteversion or retroversion, as the case may be. But sometimes, instead of altering its position in the manner described, the uterus bends, the body thus sinking without much change in the position of the cervix. Retroversion and retroflexion are thus results of a slight degree of prolapse. They are often, indeed, valuable indications of a slight yielding of the pelvic floor. There are cases in which the uterus, while the patient is in the ordinary obstetric position, is not measurably lower in the pelvis than natural, and yet we find symptoms. Did we look to the amount of descent of the uterus appreciable by the medical man as the indication for mechanical treatment, we should detect no necessity for it; but the version or flexion of the uterus reveals to us the altered condition of the uterine supports.

This, then, is the first point to which I would ask attention: that retroversion and retroflexion, when pathological, are alike results of slight yielding of the pelvic floor. Without such yielding they cause no symptoms. Their presence with symptoms is an indication of a degree of prolapse which may be too slight to be detected in the ordinary obstetric position, except by the alteration in the inclination and shape of the uterus.

The next thing to be borne in mind is that the most common cause of yielding of the pelvic floor is parturition and its effects. After delivery the pelvic floor should return practically to its condition before the pregnancy. But this is not always the case. From the injuries to the parts which attend childbearing, and from disturbances in the process of involution, some loss of tone in the pelvic floor frequently results. Hence we find prolapse, retroversion, and retroflexion, chiefly in women who have had children, and comparatively seldom in virgins. But parturition also brings with it an increased liability to many pelvic disorders: metritis, perimetritis, ovaritis, &c. These conditions are thus very commonly associated with backward displacement of the womb, simply because they are prone to occur in the same class of patients.

Anteversion and retroversion resemble one another in being alike results of slight prolapse. But the consequences of these displacements differ, for the reason that behind the uterus there is occasionally an anatomical disposition of parts to which there is nothing analogous in front of it. There are two folds of peritoneum, containing some muscular and fibrous tissue, running back from the neck of the uterus to the sacrum—the utero-sacral ligaments. These bands vary very much. Commonly they are small, wide apart, and loose; they may even not exist at all as visible folds of peritoneum. Sometimes they are so strong, tense, and close together that Douglas's pouch is a sac with a distinct and narrow neck. When this disposition of parts is present and the uterus becomes displaced backwards, its body may, by the intra-abdominal pressure acting on its anterior surface, be forced down into Douglas's pouch, against the margins of which the veins running in the broad ligaments, and returning the blood from the body of the uterus, will then be pressed. The return of blood from the uterus may thus be obstructed, and congestion of that organ be the consequence. The body of the uterus may even, there is reason to believe, be so far pushed down as to become grasped by the utero-sacral ligaments, and incarcerated in Douglas's pouch. It is rare to find these ligaments so tense and so close together as to be capable of grasping the body of the uterus. But it is not uncommon to find one or both of them quite strong enough to be capable of exerting pressure on the veins of the broad ligaments when the body of the uterus is pressed down between them.

The utero-sacral ligaments have another effect beside this. When strong and tense they tend to restrict movement of the neck of the uterus in a forward direction. Hence when, this condition of parts being present, the body of the uterus becomes displaced backwards, the utero-sacral ligaments prevent the

cervix from moving upwards and forwards. The womb therefors bends, and retroflexion is the result, instead of retroversion, which would be the kind of displacement produced if the cervix were free to move.

I would now ask permission to briefly repeat the important points in what I have been saying:—

1. Backward displacement of the uterus, when pathological, is a result and indication of slight yielding of the pelvic floor.

2. It is prone to occur in the class of patients who are most liable to inflammatory and other diseases of the pelvic organs.

3. The disposition of parts behind the uterus is sometimes such that when the uterus falls backward, the veins which return the blood from it are pressed on, the return of blood is impeded, and the uterus becomes congested.

On this view of their pathology I would classify backward displacements as follows:—

1. Those which cause no symptoms of any kind.

2. Those in which the displacement (including under that term the totality of the conditions which produce it) is the only morbid condition present, and there is no congestion, the symptoms being those of prolapse only.

3. Those in which there is not only descent, but congestion of the uterus.

4. Those in which the displacement is complicated by other conditions, which may or may not be aggravated by it.

It is difficult to get at a correct estimate, at least in this country, of the frequency with which retroversion and retroflexion occur without any symptoms; and it is not of great practical importance. Circumstances every now and then make it our duty to examine the pelvic organs, although the patient has never complained of any functional disturbance referable to them. No one can have made many such examinations without occasionally finding the uterus displaced backward; and from the few such patients whom one is required to examine, one may infer as to the many whom one does not examine that such displacements occur also in them. Further, retroversions and retroflexions have been found post mortem in patients who were known not to have any uterine symptoms during life. But the most important piece of evidence, from the large numbers of which it consists, is that furnished by Vedeler. This physician, who practises in Christiania, examined 414 healthy virgins, and found the uterus retroverted in 45 of them, or 11 per cent., and retroflexed in 13, or 3 per cent. He also examined 506 healthy nulliparæ, and found retroversion present in 47, or 9 per cent., and retroflexion in 17, or 3 per cent. (I have myself examined 111 nulliparæ who sought advice not for

uterine symptoms, but for local contagious disorders, and I found retroversion twice present and retroflexion once.) Vedeler also examined 584 healthy women, each of whom had had one or more children. Among these he found 57 retroversions, or 10 per cent., and 69 retroflexions, or 12 per cent. In all, out of 1504 healthy women, Vedeler found the uterus displaced backwards in 248, or 16·49 per cent. Out of 1158 women who were suffering from uterine symptoms, he found similar displacements present in 259, or 22·37 per cent. Other writers have published statistics showing the frequency of retroversion and retroflexion in patients not selected by their seeking treatment for uterine disease, as well as in fœtuses and children; but none have investigated such large numbers, or differentiated so carefully between the different classes of patients, as Vedeler. As evidence of his accuracy, I may compare one of his statistical statements with a similar one compiled from his own experience, by Dr. Graily Hewitt. This latter author, out of 1205 women suffering from uterine symptoms and examined by him, found retroflexion in 112, or 9·3 per cent. Vedeler, out of 1106 patients suffering from uterine disease, virgins being excluded, found retroflexion in 112, or 10·1 per cent. As retroflexion is not common in virgins, it is possible that if the few virgins probably included in Dr. Hewitt's tabular statement were subtracted from it, as they have been from Vedeler's, the result might be to make the percentage of retroflexions almost the same.

It is necessary in classifying the backward displacements of the uterus, to recognise the frequent occurrence of these cases—viz., those without symptoms; because in practice it is essential that we should always bear in mind that as retroversion or retroflexion may exist in a healthy woman without causing symptoms, so either of these displacements may be present along with functional derangement, which it has no share whatever in producing or modifying.

Cases in which the displacement produces or modifies symptoms.—The second class I would make comprises those cases in which the retroversion or retroflexion is associated only with symptoms caused by slight yielding of the pelvic floor. It is in these cases that mechanical treatment is most successful. When backward displacement of the uterus is the cause of symptoms, in the majority of cases they are of this kind, and treatment is successful in proportion as they are of this kind only. The uterus may be either retroverted or retroflexed. It may be so retroverted that it is literally upside down, the cervix being above and the fundus below: a case has been recorded in which the fundus protruded at the anus when the patient defecated. Or it may be retroflexed, being so bent

that the posterior surface of the body forms an acute angle with that of the cervix. But so long as the uterus is free to move, and its walls of natural thickness, it makes no difference whether it be bent or straight, or, if bent, whether little or much. Although at the posterior surface the bend may form an angle, there is no angle in the canal. When we examine a case of this class we find the uterus displaced in either of the ways just mentioned; but it is quite movable, neither body nor cervix is enlarged, and there is no tenderness. The only symptoms the patient complains of will be a pain in the sacral region, and also more diffused pain, referred to the loins, lower abdomen, and thighs, and described as a dragging, bearing-down pain, a feeling as if something were falling from her. All these uncomfortable sensations disappear when the patient lies down—that is, when the intra-abdominal pressure is taken off; a point upon which I would lay particular stress. Generally the patient will complain of having to micturate with undue frequency. The menstrual function will be unaltered. There will often be leucorrhœal discharge, because the conditions (subinvolution, &c.), which weaken the pelvic floor, also favour the occurrence, and retard the disappearance, of vaginal catarrh. It is also very common to hear of constipation and dyspeptic symptoms, dating back to a time when the patient was free from uterine trouble. This is because the straining to pass scybala, which results from constipation, is a frequent cause (generally co-operating with others, sometimes perhaps alone) of these displacements. It is in these cases, in which the symptoms are those, and those only, which slight prolapsus produces, and in which the only morbid condition present is the displacement (including under that term not only the alteration in the position and shape of the uterus, but the changes giving rise to it), that mechanical treatment is most brilliantly successful. A pessary which pushes up the uterus relieves the symptoms at once. It matters nothing, in this class of cases, what the effect of the pessary upon the shape of the uterus may be. It may straighten the uterus or leave it bent; but if it pushes the uterus up, and remains in its position without injuring any part by its pressure, it will relieve.

3. The next class of cases includes those in which there is not only displacement, but congestion of the uterus. This congestion may be either a result, a cause, or an accidental accompaniment, of the displacement. When the body of the uterus sinks down between well-developed utero-sacral ligaments, its veins, which run in the broad ligaments, will, if the uterus be forced down strongly enough, be compressed against the utero-sacral ligaments, and the return of blood from the uterus thus be impeded. In such cases the displacement causes the congestion.

There is also reason to think that by congestion of the uterus and the other structures having the same vascular supply, the pelvic floor may become so relaxed and weakened as to allow the uterus to fall backwards. In these cases the displacement is the result, not the cause, of the congestion, the relation being shown by the fact that when the parts have returned to a normal condition the displacement disappears. Thirdly we have no ground for supposing that retroflexion of the uterus confers any immunity from uterine congestion, and, knowing that retroversion or retroflexion exists in a certain proportion of healthy women, it is obviously possible that they, as well as those whose uteri lie more nearly in the axis of the pelvic brim, may become the subjects of uterine congestion, which yet has nothing to do with the displacement, either as cause or as effect, the relation between the two being simply that of coincidence.

It is extremely difficult to estimate in what proportion of cases congestion accompanying backward displacement is either its effect or its cause. Recovery while mechanical treatment is being used, or relief following its application, does not prove that the congestion is the result of the displacement; for the symptoms of prolapse may be relieved by mechanical support, in cases in which the congestion is quite independent of the displacement.

In these cases the symptoms are those of prolapse *plus* those of congestion. The pain is greater and is not immediately relieved by lying down. There is hemorrhage, the menstrual flow is increased in quantity, prolonged, or recurs with undue frequency; and is attended with more than usual pain. The body of the uterus is swollen, tender to the touch, and there is pain on defecation. Micturition is attended with cutting or burning pains, and there is irritability of the bladder. It has seemed to me that pain and hemorrhage in these cases (as, indeed, in some other kinds of uterine disease) stand to some extent in antagonism to one another. When there are abundant losses of blood, the congestion is thereby diminished, and the pain is less. When uterine hemorrhage is not more than is customary for the patient, pain and tenderness are often very pronounced symptoms.

In cases in which congestion is the result of the displacement, it can be cured by raising the uterus, so as to relieve the broad ligaments from pressure. This is most efficiently done by a pessary which anteverts the uterus, or at least keeps it in the axis of the pelvic brim. A Hodge's pessary will often do this, and when it does, it removes the symptoms with striking rapidity and completeness. But the Hodge's pessary is formed of a thin and hard bar. If it fail to antevert the uterus—if, as

it is represented in some books as doing, it presses directly on the tender congested uterus,—it will aggravate instead of relieve the symptoms. This is an essential and practical difference between this class of cases and the one last mentioned: in the former, anything that pushes up the uterus will relieve, whether it does so by directly pressing on it or not, and it matters nothing whether that organ is straightened or remains bent. In the latter a thin hard pessary, if it presses directly on the tender uterus, will do harm; to do good it must antevert it; and if a retroflexed uterus be anteverted, the flexion will be removed. Hence relief often coincides with the removal of flexion, although that condition has nothing to do with the production of the symptoms. But often it is difficult, and sometimes impossible, to get a Hodge's pessary to antevert the uterus, and we find the best fitting instrument that we can adjust only raises the body of the uterus by pressing directly upon it, and consequently causes discomfort. In such cases I have found the best instrument to be a thick indiarubber ring, which, it is true, presses on the uterus, but, being thick and soft, its pressure is bearable, and as it raises the uterus, it relieves, although not so thoroughly as an instrument which keeps the organ anteverted.

Lastly, we have cases in which the displacement is combined with other morbid conditions of which it is neither cause nor effect. Backward displacements of the uterus are, as I have already stated, more commonly associated with morbid phenomena than is anteflexion, because the injuries consequent on childbirth which predispose to inflammatory disorders of the uterus and its adnexa, also predispose to prolapse and backward displacement. In such cases the symptoms may or may not be modified by the displacement. If there be any degree of prolapse, or if there be congestion, the symptoms so caused will add to the patient's suffering. Prolapse implies increased mobility of the uterus; movement of the body of the uterus involves movement of the Fallopian tubes and ovarian ligaments, and movement of these parts will be communicated to the ovary. Movement of inflamed parts produces pain, and, therefore, if ovaritis, salpingitis, or perimetritis be present, abnormal mobility of the uterus, by producing movement of the inflamed part, will aggravate the pain. Treatment which keeps the uterus in one position, and so limits its range of movement, will lessen the pain. It may thus happen that mechanical support in a case of backward displacement of the uterus will relieve symptoms not produced by the displacement. These form a group of cases that must be recognised. In them we cannot promise the immediate and complete relief which mechanical treatment gives to symptoms entirely dependent

upon a mechanical cause, although sometimes the patient's discomfort may be lessened by a pessary. This group of cases, it will be obvious, comprises diverse combinations of morbid conditions. It can only be defined as including those cases of backward displacement of the uterus which do not come under either of the three former categories.

To sum up the purport of this paper. Its object is to classify cases of backward displacements of the uterus in a manner which shall assist us in their treatment, to divide them into groups the differences between which are real and practical. The classification suggested is the following, its application in practice being at the same time epitomised:—

1. *Retroversion and Retroflexion without symptoms.*—In probably the majority of cases of retroversion and retroflexion there are no symptoms arising from the peculiarity in the shape or position of the uterus. Therefore, in order that this displacement shall call for treatment, it is not enough that the patient shall complain of something, but the symptoms must be such as the displacement is known to produce—*i.e.*, symptoms of which we can predict relief by mechanical treatment.

2. *Retroversion and Retroflexion without Congestion.*—In most of the cases in which backward displacement of the uterus is the result of morbid changes the symptoms are only those of a slight degree of prolapse. Such cases are at once relieved by an instrument which pushes up the uterus. It matters nothing what is the shape of the uterus, or what the effect of the pessary in altering its shape. In proportion as cases approach this simple type, so is the certainty of relief from mechanical treatment.

3. *Retroversion and Retroflexion with Congestion.*—In a smaller number of cases of backward displacement of the uterus there is also congestion of that organ, the symptoms of which condition are then added to those of simple prolapse. In such the pressure of a hard pessary upon the body of the uterus will aggravate the patient's discomfort, and the maximum of relief will only be gained by a pessary which anteverts the uterus. In some few cases the congestion is the direct result of the displacement, and in such immediate and complete relief will follow anteversion of the uterus.

4. *Retroversion and Retroflexion complicated with other Morbid Conditions.* — Many cases are complex, the displacement coexisting with other morbid conditions. It is only in the simple cases that benefit from mechanical treatment can with certainty be predicted.—*Lancet, Dec.* 9 *and* 16, 1882, *pp.* 980, 1028.

83.—ON A NEW METHOD OF TREATING DISPLACEMENT OF THE UTERUS.

By William Alexander, M.D., F.R.C.S.Eng., Surgeon to the Liverpool Workhouse Infirmary.

During the years 1879, 1880, and 1881, the gynecological wards of the Liverpool Workhouse were infested by a swarm of patients, every one the subject of some uterine displacement. Prolapsus uteri was the commonest variety, but there were also many cases of retroflexion, and a few were cases of anteflexion. Every surgeon has noticed such apparent epidemic occurrences at times of non-infective disease. All of these patients had been under treatment for years. Pessaries were either useless, owing to the aggravated form of the displacement, or, if useful, were too irritating either to the mind or to the body of the patient. When a pessary would seem to fit, and the patient was discharged, the chances were that she would return in a few days with the pessary in her pocket, or she would be admitted drunk, and the pessary gone.

Nearly two years ago I thought of the round ligaments as a means for keeping up the uterus, but my impression of their possible utility was so small, that until June or July of 1881 I never examined them. At that time, however, I got the body of an old woman for *post-mortem* examination, and was astonished to find how thick and strong these ligaments were in her. For three or four months I examined all the round ligaments I could get my hands upon, and on December 1, 1881, I successfully operated on a case of prolapsus uteri. On April 1, 1882, I published four cases on which I had operated (*vide Retrospect*, vol. 85, p. 280). Since that time I have operated on six more cases.

I have learned a good many practical points as to the performance of the operation since April last, which I will now describe, and shall illustrate the steps of the operation by this subject, in which I have exposed the round ligaments :—

The pubes are shaven on each side, from the spine of the pubes outwards. The pubic spine is felt with the finger, and an incision made upwards and outwards for two or three inches in length, in the direction of the inguinal canal. The greater or less length depends on the amount of fat that covers the abdominal parietes. By subsequent incisions the depth of the wound is increased until the tendon of the external oblique muscle is exposed. The external abdominal ring will now be seen, or some vertically directed fibres that run across it will act as the guide to it. In the first incision a small artery (the superior external pubic) is cut across, and should be tied. It is the only vessel in danger, and afterwards the operation is bloodless. The

vertical fibres that cross the external abdominal ring should be cut in the direction of the wound, when a reddish-white tissue bulges out, in which is the termination of the ligaments. The final attachment of the ligaments here, there, and somewhere else, are anatomical questions that need not trouble us. All we have got to do is to put an aneurismal needle below the mass of fatty tissue, and raise it up, so that it can be caught by the fingers, and not by forceps. By gentle and careful traction the mass of fatty and ligamentous tissue is pulled out, and the thick ligament is exposed. The small nerve that accompanies it should be cut through, as well as any adhesive bands that bind it to neighbouring structures, until the naked ligament is held in the fingers. When this has been accomplished, the uterus will be felt to move by traction on the ligament, and can be lifted up to the abdominal wall. Having freed the opposite ligament to the same extent, and by the same means, the important question arises as to how far we are to pull these structures out.

If the case is one of *retroflexion*, the uterus should be put by the finger or uterine sound exactly into position, and the ligaments pulled upon until they have *control over that position*. It is not only unnecessary, but irrational, to cure the abnormal position by another as abnormal only in an opposite direction. The uterus should be placed in position by the sound, and not by traction upon the ligaments, because traction upon the ligaments alone would, in the first instance, tend to produce prolapse, especially if the retroflexion was a profound or inveterate one.

If the operation is performed for *prolapse*, the ligaments should be pulled much further out, so that the uterus just touches the rectum, and only rests slightly upon it. If we draw the uterus up further, and if it is very heavy, its whole weight will be upon the round ligaments. These will stretch, and they may even yield altogether, so that no better, or a much worse, result may be obtained than if we pulled the womb a shorter distance, leaving it some support from the deeper pelvic structures. The number of inches of "slack" pulled out is no guide. The position in which the uterus should be retained is the true guide.

Method of retaining the Ligaments in their new position.—This has given me more trouble than anything else. I have employed wire ligatures, clamps, silk ligatures, and cat-gut ligatures. I have cut the "slack" off; I have tied the "slack" of one ligament across the middle line to the "slack" of the opposite ligament. I will only give you the method that I believe to be the best, as the details of my experience would only weary you. You will observe in this dissection that the "slack" of

the ligament is continuous at one end with the intrapelvic portion of that structure, and at the outer end with the various bands that spread in various directions, and that are lost in the fascia of the mons veneris and its neighbourhood. I pass a needle, armed with a strong cat-gut ligature, through the skin at the outer and lower side of the wound, through the outer pillar of the external abdominal ring, through the round ligament just as it emerges from the depth of the wound, through the inner pillar of the external abdominal ring, and through the skin on the opposite side of the wound to that in which the needle entered. The ligature is pulled through, and knotted in the first coil of the usual surgeon's knot. The depth of the wound is then sponged effectively, the sponge suddenly withdrawn, and the ligature immediately drawn tight, so as to bring the parts into apposition without unduly constricting them. Another ligature is put on in the same way, and perhaps a third, and then the rest of the "slack" is loosely packed in the inner part of the wound, and retained there by cat-gut ligatures. The object to be attained is the retention of the ligaments in position without any undue constriction. By rough handling, or by the excessive constriction of ligatures, the ligaments are made to slough, and there will consequently be a much greater risk of failure and of wound trouble.

The operation is one that can be undertaken under any system of surgery. I use the spray, and the "spray-gauze" men may use all their appliances. I do not employ drainage now, and I merely apply over the wound a purified sponge covered with gauze, firmly kept in position by a spica bandage. Any simple dressing may be used afterwards. Three weeks is the time for which private patients should make arrangements, and during which they will be compelled to rest.

It is extremely difficult to get the wounds to heal by the first intention, the necessary handling of the ligaments and the tension of the stretched ligaments upon their ligatures, are the probable causes of the difficulty. I would advise, and I intend to follow the advice myself in future, an intravaginal support during the decubitus of the patient, as likely to promote the healing of the wounds by relieving the tension. Nervousness and restlessness in patients should be subdued by the usual means, and getting out of bed for the first fortnight should be absolutely forbidden. The usual causes of prolapse should be avoided for some time after the operation, as the ligaments may slip under violent strains, even when the cure is supposed to be complete.

What are the cases to which the operation may be applied?—It may be applied to all cases of retroflexion or prolapse where no obstacle exists to the restoration of the womb to its normal

position. Of course, whether it should be applied to many of these cases will depend on circumstances peculiar to each case. I do not wish to recommend it as the mode of treatment in all cases of prolapse or backward displacements, and the natural repugnance of patients to operative methods of treatment will limit it to those where life is a burden, through the irritation or inefficacy of other means. Where strong pelvic adhesions exist, that render the replacing of the uterus in position impossible, the operation could not be entertained. Even in cases where the pelvic adhesions are not so strong the round ligaments may be so adherent as to be incapable of being pulled out.—*Liverpool Medico-Chirurgical Journal, Jan.* 1883, *p.* 113.

84.—ON PESSARIES IN MINOR DISPLACEMENTS.

By J. Matthews Duncan, M.D., LL.D., Physician-Accoucheur to St. Bartholomew's Hospital, London.

In the present great abundance of contorted bits of wood, and metal, and vulcanite, and what not, called pessaries, my advice to you is Punch's advice to a young man contemplating marriage — Don't! Think twice before beginning the often baneful practice of using any instrument, teaching a woman to depend on what, if not positively useful, is positively injurious, though perhaps not much so. Many a woman has suffered from, and many a woman has died of, a pessary; but most pessaries, as I find them in use, are nearly innocuous for evil or for good. They are always harbourers of dirt, and they always keep the mind watching the part; they are all liable to decay, and require, if long used, to be renewed. They all are undesirable additions to the contents of the pelvic excavation, and, if they are efficient, must, of course, cause more pressure, though perhaps on different parts, than that caused by the organ or organs which they keep in an altered position.

Pessaries are used for the purpose of keeping up replacement of descended or otherwise displaced organs, or of displacing the organs and keeping them displaced, or of fixing or nearly fixing them against the results of succussion or shaking; and all these come into the one category of mechanical objects. But you sometimes see what are called galvanic pessaries, whose object is to act otherwise than mechanically, giving a homœopathic dose of galvanism. These galvanic pessaries are used in amenorrhœa and in virgins; and to all this kind of meddling there are strong objections, medical and moral. Till you know something more precise in its favour than the vulgar talk of "cures" you should have nothing to do with it. Look upon pessaries as a surgeon looks on a truss, not medicinal otherwise than as a mechanical means of procuring healing, comfort, and safety to your patient.

Speaking of virgins, I may say that there is very rarely occasion to examine such for displacement, and that, when examination is made, it can generally be done quite satisfactorily per rectum. You get the knowledge of the condition of the pelvic viscera that you want, and that is all you should require. If you find only a minor displacement you had better let it alone, not even trying a pessary. It is only in very rare complicated cases with distinct mechanical indications that a pessary should be tried or used. I do not remember myself using one on any ground whatever in a virgin.

Intra-uterine or stem pessaries are the only instruments you can rely on for straightening the uterus, or keeping a flexion undone. They do this as a male bougie straightens the urethra. Some kinds have an outside or pubic part by which the straightened uterus is fixed; but the oldest kind and the most recent respect the mobility of the uterus. They have been three times introduced into practice within this century, but the practice has never flourished. Many modifications have been ingeniously devised with a view to perfect them, but in vain. I do not expect they will ever find occupation in the conditions now under discussion. They are far more injurious and dangerous than the conditions they are intended to modify. There is no such instrument in "Martha."

The evils of intra-uterine pessaries have led to great ingenuity in attempts to undo flexions and keep them undone by vaginal, not intra-uterine, instruments. This attempt is often successful in retroflexion which does not occur as a congenital rigidly fixed condition, and can be dealt with just as a retroversion is managed. But the curious things are anteflexion pessaries; and in regard to their giving relief I meantime express no opinion; but I do say that if they give relief it is not by undoing the flexion and keeping it undone, keeping the womb straight. I have seen most kinds of anteflexion pessaries as placed by their inventors, and too often replaced and replaced, but I have never seen one materially modify the flexion. I have myself never used one, and have no intention of doing so. There is another bad and too common practice which I must not omit to mention here, that is, what is called straightening or putting up the womb or replacing it time after time by the probe or finger. This has no other effect than to irritate the organ, for the displacement recurs immediately after the probe or finger is removed, as the practice itself shows.

It is not a simple matter to judge of the part taken by a pessary in relieving or removing painful symptoms. A kindly doctor makes an amiable patient anxious to please him and ready to express a sense of relief which may not be real. Besides, you will find many patients alarmed at the idea of

having a displacement, and, believing the pessary undoes it or cures it, wear an instrument with satisfaction and even pleasure, although it gives them new pains or increases what they had before. Such patients live in the pleasing and sustaining delusion that the pessary is curing them, and object to its removal even when removal gives relief, and although told that the pessary, when in, does not alter the condition of displacement. In such difficulties how are you to be guided? The difficulty is almost insuperable if your patient has become possessed by erroneous notions of the importance of displacements; and you must take care to prevent the adoption of such notions.

You must guide your patient's mind aright, and take care of the displacement, acting on two principles—first, not to allow harm to come through your treatment; and second, that practice overrules all theories in the present imperfect state of our knowledge; that is, if your patient gets real relief from any kind of pessary that does not do important harm, let her have that relief.

This leads me to enter more carefully on the question—What do you expect from a pessary?

You may replace a descended or retroflexed or retroverted uterus, and keep it replaced by a pessary, and you may so relieve or remove pains. You cannot cure a displacement, though sometimes you can substitute one displacement for another, that is, for example, change a retroversion into an anteversion. No doubt a displacement may sometimes be, in a sense, cured—as when an adhesive perimetritis ends in tying a uterus up to the higher part of the sacrum. But all kinds of minor displacements are incurable by any kind of instrumental treatment. Remove the instrument, and the displacement is just as it was before, or there is a new alternative one, and this, however long the instrument may have been in place.

Displacements sometimes disappear, or are cured spontaneously or by aid of proper treatment. Thus a woman with chronic inflammation of the cervix, and probably also relaxation of the vagina, gets rid of these conditions, and then the uterus ascends from its descended and perhaps flexed position. A woman with a bulky uterus, perhaps containing a small fibroid, becomes aged: the uterus becomes lighter and lighter, and the upper vagina contracts, and the descended uterus ascends. Any change in the constitution of the abdomen which increases its retentive power will raise the uterus higher, destroying displacement, and such changes in the abdomen may result from enlargement of the base of the thorax, or from changes in the quantity and disposition of fat.

I have already said that a pessary often cures by its effect on

the mind. A patient recently said to me, "You have quite cured me. I can walk now, but not without that pessary." And she was not altogether pleased when I told her she had no pessary,—that I had removed it months previously without her being aware of my having done so. I had omitted to tell her. Had she known she had no pessary she would have found pains arise from walking, and all this without any desire to be untrue.

A pessary often gives relief, even when small, and having no discoverable function, doing nothing, Of the occasional occurrence of such cases I do not doubt, and I am quite unable to explain them. It is of such cases I was thinking when I told you that practical success must overrule theory, or take the place of a failure in theory. It is quite common to find a pessary give relief in what may be called a flexion, because that feature of the case is most striking, without the pessary changing the flexion. In such cases the pessary may maintain a diminished degree of descent, and may prevent increase of descent on walking and may save a tender part of the uterus from pressure on sitting. There is no difficulty in explaining such cases; but to comprehend the action of the pessary you should think of the case as one of descent—not of flexion; and this is true of almost all—if not all—cases of flexion.

As a matter of fact, I find the majority of versions and flexions, as observed in practice and treated by pessaries, have their whole conditions of displacement quite unaltered by the pessary, even while in.

One of the best examples of relief by a pessary is observed in the anteversion (by probe) of an engorged retroverted and descended uterus. Here a well-fitted Hodge is comforting and curative, maintaining the anteversion, elevating the uterus or preventing descent on walking or standing, and preventing relapse into retroversion or retroflexion by keeping the posterior laquear of the vagina pressed against the sacrum.

Another notable example of relief is seen in descent with tendency to cystocele, when the irritation of the cystocele pushing at the orifice of the vagina is most annoying. In such, a suitably sized Hodge, or india-rubber ring, often, by its anterior limb, just catches the cystocele and obviates the tendency to protrusion through the os vaginæ.

For each case your pessary must be specially adapted—a boat-shaped or a double-curved—and it must fit the patient in size and contour. Nothing can instruct you in this but bedside experience. Occasionally you have to try more pessaries than one to find the most suitable. Sometimes a woman, whose case you expected to relieve by pessary, can bear none of whatever kind.

A pessary, if it is to be useful, will give relief at once, and will need very little attention from you. If you are frequently fitting and re-adapting, you are almost surely doing more harm than good. A well-fitted pessary may be worn for months without being attended to. You must take care that the pessary does not cause ulceration and cut the vagina, and you must have a new one placed when the former one gets decayed.

You will find it hard to get any good from a pessary unless you have a fair amount of perineum to support it. A pessary will be inefficient if the vagina is not long enough and capacious enough to allow of its action without strong pressure on the vaginal wall.

In flexion or version, without descent of the whole organ, you can do no good to the version or flexion by a pessary: you have no basis or fulcrum to work from.—*Medical Times and Gazette, Dec.* 30, 1882, *p.* 769.

85.—MANGANESE IN THE TREATMENT OF AMENORRHŒA.

By Dr. SYDNEY RINGER, Physician to University College Hospital, and Dr. WILLIAM MURRELL, Physician to the North-West London Hospital.

We are desirous of calling attention to the value of a very simple remedy in a very common complaint. For some time past we have used permanganate of potash with much success in the treatment of certain forms of amenorrhœa, and are satisfied of its value. Our observations have extended over a period of thirteen months, and we have now notes of sixty-nine cases. The majority occurred in hospital practice, but some were private patients. A small number remained under observation for a few weeks only, but the majority continued to attend for a much longer period; so that in some instances we have a complete record of the menstrual history for a year or more. In some cases the amenorrhœa was the cause of the patient seeking advice; in others its existence was mentioned incidentally, the patient suffering from some other complaint. Our cases are such as come under the care of the general as distinguished from the obstetric physician, and do not include those requiring operative interference. As a rule we refrained from making a vaginal examination, but with this exception our notes are complete. We have used the permanganate in two forms, first the pharmacopœial solution, and secondly the permanganate made into pills, each containing either one or two grains. Generally we begin with a grain three times, and then gradually increase the dose to two grains four times a day. Our most striking results have been obtained with the larger doses; a

large dose sometimes succeeding admirably after the failure of a small one. Before commencing treatment we inquire carefully into the menstrual history of the patient, and as a rule give the remedy only for the three or four days immediately preceding the expected period, but should it fail to produce the desired effect we direct the patient to continue steadily taking it, and in some cases it has been taken continuously for nearly three months. In our experimental observations we have given the one drug only and have done nothing in the way of accessory treatment. Our most striking results have been obtained in young women between the ages of eighteen and twenty-five, who from some accidental or trivial cause, such as catching cold or getting wet, have "missed" once or twice after having been regular. The administration of one or two grains of permanganate of potash in pill three or four times a day for a few days before the time of the expected period will bring on the flow almost to a certainty. In some instances the periods were brought on after the patient had ceased menstruating for over a year. In the case of country girls who have "seen nothing" for a month or two after coming to town the treatment has answered admirably. Often enough patients do not consult their doctor until they are "overdue," until the time of the expected period has passed by for some days. Even then the prompt administration of the permanganate will often bring on the flow at once, but should it fail to do so the treatment ought to be continued, and the patient will probably menstruate normally at the next monthly time. Generally our efforts are not crowned with success until the medicine has been taken for at least three or four days, but in some instances the permanganate acted with striking rapidity, the menstrual flow making its appearance after only two or three doses had been taken. It is not necessary to discontinue the treatment on the appearance of the menses; in fact we generally tell the patient to continue taking the pills three or four days longer, finding that it facilitates the flow. The permanganate often succeeds well after the failure of other remedies, such as iron, aloes, nux vomica, strychnia, pulsatilla, nitro-glycerine, and hot-mustard baths. Sometimes, however, it is necessary to give it for six weeks or even longer before the desired result is obtained. In those cases where the patient has menstruated only once or twice, and has then entirely ceased for some months, our treatment answers well; the menstrual function is re-established, and thenceforth proceeds normally at every successive monthly period. In some cases there was no actual amenorrhœa, but the flow was scanty, lasting perhaps only a single day, or it may be only a few hours. Here the administration of the permanganate prolonged the flow, and even in some instances when it had ceased brought it on again.

In girls of about fifteen or sixteen, who have never menstruated at all, the permanganate, as might be expected, is not so certain in its action; but even here it not infrequently acts promptly, bringing on the flow at once. In some cases where the general health was bad, and the permanganate had failed, we suspended treatment for a time, and sent the patients into the country for a month. On their return we gave the permanganate a second trial, and it succeeded at once. We have, however, sometimes failed to bring on the menstrual flow even when the patient was in fairly good health, and when there were the usual indications of puberty.

It is not only in the case of young women that manganese is so useful, it succeeds almost equally well with women between thirty-five and forty, who, as the result of many pregnancies and much suckling, have ceased to be regular. Here, for example, is a typical case. A married woman came to us complaining that she was never regular. She had had nine children in as many years, and rarely saw anything more than once between her pregnancies. She had been suckling for eight months, and had not been poorly for seventeen months — the nine months she had carried and the eight months she had suckled. She was not in the family-way, but said she expected she would be soon if she weaned the baby. She did not know when she ought to be poorly, and had given up all expectation of seeing anything. She was ordered two one-grain permanganate of potash pills four times a day, and came on poorly a fortnight after, the first time for seventeen months.

We need hardly say that before treating the amenorrhœa care should be taken to see that the patient is not pregnant, although we are satisfied that the permanganate given in the dose we recommend has no power to produce abortion either in the early or late stages of pregnancy. We find that manganese fails to induce the flow when the amenorrhœa is due to advanced phthisis. But in some cases of arrested phthisis the treatment was successful, and the patient, after a time, under the influence of the permanganate, menstruated freely and at regular intervals. In several instances patients informed us that the pills had proved of value in curing "whites" of long standing. As a rule the permanganate is taken without difficulty, but patients much prefer the pills to the solution. The solution is peculiarly disagreeable to take, and in some cases produces nausea and even vomiting. Patients frequently complained after taking the pills of a heavy persistent pain over the upper part of the sternum, "as if something had stuck there and would not go down." This was not due to the drug being given in the form of a pill, for the same complaint was made when the same dose was given in solution. One patient said the pain was of a burning character, and another said it

was like heartburn. A girl of sixteen, to whom two two-grain permanganate of potash pills were given four times a day, said the pain, "like a lump at the chest," came on immediately after each dose, and was so intense that she had to go to bed for two hours.

That the effects we have described are due to the manganese, and not to the potash in the salt, is shown by the fact that manganate of soda and binoxide of manganese are equally efficacious in the treatment of amenorrhœa. The manganate of soda was given in two-grain pills, two four times a day; and the binoxide in four-grain pills, one four times a day. It may be thought that the manganese acts by improving the condition of the blood, but this is not the case. The treatment succeeds equally well in the plethoric and in the anæmic. Given in cases of chlorosis, the permanganate not infrequently brings on the period without in any way improving the anæmia.—*Lancet*, *Jan.* 6, 1883, *p.* 7.

86.—ON TRANSFUSION—FOR THE HEMORRHAGES OF PARTURITION.

By Chas. Egerton Jennings, L.R.C.P.Lond., late Resident Accoucheur and House-Physician at the London Hospital.

The earliest attempts at, and experiments on transfusion—which, from time to time, have been corroborated by actual practice as well as by repetitions of them—conclusively showed that if an animal lose a large quantity of blood the loss *per se* can be successfully supplemented by transfusion from an animal *of the same species*. Out of a large series of cases it is found that the operation was performed in nearly one-half of them for the acute anæmia caused by ante-partum and post-partum hemorrhage. A positive result was obtained in two-thirds of this section of these cases, and more than half of them terminated in complete recovery. The accoucheur must, therefore, consider the operation imperative whenever life is jeopardised by severe ante-partum or post-partum hemorrhage, the hemorrhage itself having been arrested.

Of all the methods of transfusion, that one which is theoretically the most perfect—immediate transfusion—is practically of the least value, for no means have yet been found to securely provide against the formation of coagula in the apparatus. All the varieties of this method involve an appreciable risk to the donor of the blood, and owing to the complexity and gravity of the double operation, it, as a rule, should only be attempted by a skilled surgeon, with skilled assistance and hospital appliances at his disposal; hence it is only feasible in the minority of the cases of the class under consideration in which transfusion is demanded.

It is of importance to remember that it is the *dynamic* rather than the nutritive value of transfusion which is serviceable in combating acute anæmia. This is well exemplified by the success which usually attends saline intravenous injections (employed as substitutes for blood transfusion). From a perusal of the literature on the subject, from the experience of Dr. Little, when cholera was rife, that saline alcoholic intravenous injections were well-tolerated and beneficial, and from my personal knowledge of this plan of procedure, I am convinced that for combating the effects of alarming hemorrhage it is an almost perfect substitute for blood transfusion, and, indeed is, in many respects, superior to that method, for it is always readily applicable, whilst the latter is not; the saline fluid can be procured *ad libitum*, whilst the supply of blood is always very limited, and the risks special to blood transfusion clearly do not exist.

I have published the notes of a case in which, with the best result, sixteen ounces of a saline alcoholic fluid were injected into the median basilic vein of a patient the subject of profuse ante-partum hemorrhage, the method being adopted not by election, but by compulsion, for although in London, and on a Sunday afternoon, a blood-giver could not be obtained *at a moment's notice*.

To provide a simple and safe and convenient and inexpensive means for intravenous injection, Messrs. Maw, Son, & Thompson have made for me an instrument which consists essentially of five feet of rubber tubing, to be used after the principle of the syphon or nasal douche, with a bulb for exhausting the air contained in it prior to use; the tubing terminates at one extremity in a bell-shaped aperture, guarded by a metallic grating; at the other in a stopcock, to which a cannula can be "plugged on." Where the syphon passes over the edge of the receptacle the rubber is protected from pressure by a semi-circular canal of vulcanite through which it passes. It has been objected that the fluid, in passing through five feet of tubing, will lose much heat. I find, however, owing to the fact that india-rubber is a bad conductor, only a fractional part of a degree of heat is thus lost. The temperature of the fluid can be estimated sufficiently well for practical purposes by inserting a thermometer in the receptacle, or with unerring exactitude by means of a spirit thermometer constructed in the wall of a small glass tube which interrupts the continuity of the rubber near the cannula, and which is necessary for the purposes of observation. The cannula is serpentine in form, and compressed laterally and engraved where it should be grasped by the operator's fingers, that it shall not slip. It tapers gradually to a point, to afford ease of introduction into the opened vein; a point, however, not sharp enough to perforate the walls of the vein whilst lying therein. The aperture for the egress of

the fluid is half-an-inch distant from the point of the cannula, that it may be closed by the pulp of the accoucheur's index finger to prevent the escape of the fluid whilst the point of the instrument enters the vein.

A great advantage will be gained by isolating and ligaturing the vein directly it has been fully exposed. The wall of the vein should be grasped with a forceps (to the proximal side of the ligature), and then punctured with the scalpel. By these means any flow of blood from the distal direction will be prevented, and the cannula can be inserted into the opened vein as directed. To open the vein with a scissors, as recommended on high authority, instead of with the scalpel, is a needless complication, tending to waste valuable time which the use of an additional instrument must necessarily absorb. I would most strongly urge that the precaution of securing the cannula within the vein by means of a (second) ligature should never be omitted. This ligature will most effectually exclude air from, and prevent the cannula from slipping out of the vein, an accident which has frequently happened, even when assistants have been delegated to maintain the cannula *in situ*.

The instrument is contained in a case which also includes a scalpel, aneurism needle, and dissecting forceps with fine points, a bottle for absolute alcohol, some saline powders, and needles and ligatures. If two drachms of alcohol be added to a pint of water at 100° F., into which a powder of the following composition has been stirred, the obstetrician is at once provided with a means, similar to that adopted in the case quoted, of combating acute anæmia:—Chloride of sodium, 50 grains; chloride of potassium, 3 grs.; sulphate of soda, 2·5 grs.; carbonate of soda, 2·5 grs.; phosphate of soda ($Na_3 PO_4$), 2 grs.

Since this syphon has been adopted at the London Hospital it has there been successfully employed, and the advantages I have claimed for it have been practically demonstrated. It is applicable not only for the intravenous injection of salines, but also of defibrinated blood, if obtainable. By a simple modification of this instrument (which will shortly be published in a monograph now in the press) it may also be used for immediate blood transfusion combined with saline intravenous injection—the favourable conditions essential for the due performance of the double operation being present—which, inasmuch as it embraces in the highest degree both the *nutritive* and *dynamic* advantages of transfusion, must be regarded as the most perfect of all the methods with which we are familiar. The chief objection to immediate transfusion as now practised —the danger of fibrination in the apparatus—will, by the modification of the instrument I have devised, be abolished, and the risk accruing to the blood-giver will be minimised.—*Medical Press and Circular, Jan.* 3, 1883, *p.* 4.

ADDENDA.

87.—ON THE DEATH-RATE OF ANÆSTHESIA, WITH AN ACCOUNT OF SIX FATAL CASES.

By W. Roger Williams, F.R.C.S., Surgical Registrar to the Middlesex Hospital.

During the last few years, an impression has arisen in the profession, and has been communicated to the general public, that the induction of anæsthesia is a much more dangerous proceeding than was formerly believed. The numerous fatal cases lately recorded in the journals have proved, in the clearest manner possible, that this impression is only too well founded on fact.

My own experience, which there is no reason to regard as singular, leads me to believe that the real dangers of this proceeding are still very greatly underestimated. The majority of fatal cases are hushed up; only a few find their way to the journals. Six deaths from this cause, none of which have been made public, have come under my immediate notice during ten years of hospital work.

On an average, I have, probably, witnessed the induction of anæsthesia about four times a week, during the whole of this period—an estimate over, rather than under, the mark. Hence, the mortality in these cases may be roughly stated at 1 in 350 inhalations. But it should be remembered that this does not represent the whole of the danger, for, in a still larger proportion of cases, alarming symptoms supervened, and fatal results were only narrowly avoided. As far as I can judge, the number of times that chloroform and ether were severally administered was about equal. Five deaths resulted from the use of the former agent, and one from that of the latter. In the former category, I have included three fatal cases, due to the use of a mixture of two parts of chloroform with three parts of ether; because, as an anæsthetic agent, the action of this mixture precisely resembles that of chloroform.

Calculated on this basis, then, the death-rate for chloroform would be 1 in 208 inhalations; and that for ether, 1 in 1,050—the lowest estimate possible. Although the total number of cases on which this statement is based is not so large as, under the circumstances, might be desired, especially as regards ether, I think, nevertheless, it may be accepted as conveying a more

correct idea of the dangers actually involved, than any of the statistics usually quoted.

In the Journal of December 30th, 1882, the death-rate for chloroform is given as 1 in 2,873 inhalations, and that for ether as 1 in 23,204—a ridiculously low estimate. Perhaps some of your readers will be able to explain these remarkable discrepancies, and to decide which statement most nearly approximates the truth. I ought to add that my field of observation has been a wide one, not confined to one institution or one method, but comprising several metropolitan and provincial hospitals; hence I think it may fairly claim to be typical of the kind of practice it represents.

I now propose to give a short account of each of these cases, and then to conclude with a few additional remarks.

Case 1.—This patient was a stout, well-built, florid man, aged 37, rather above the usual height. He was in a robust state of health at the time of the fatality; and he had previously been of moderately temperate habits. He was sent from the out-patient department to the operation-theatre, to have a small piece of dead bone removed from the end of the stump of his right forearm. The limb had been amputated a little below the elbow-joint, about a year previously, for a bad crush involving the hand, wrist, &c.; and anæsthesia had then been induced without any trouble, by the gas-and-ether method, with Clover's apparatus.

For an operation of such a trifling nature, gas alone was deemed sufficient, and he was nearly under its influence, when it was found that the supply in the reservoir was exhausted. Chloroform was now sent for; and, during the delay thus involved, the patient completely recovered consciousness, and said he thought he had been for a ride in the train. About a drachm of chloroform having been poured without measurement on to a piece of folded lint, the administration was effected by holding this rather closely over the patient's mouth and nose. To prevent the dissipation of the rising vapour, a large towel was thrown over the patient's face, as well as over the administrator's hand. By this means, after a well-marked stage of muscular excitement, sufficient anæsthesia was induced in *less than three minutes*. Immediately after the first incision, it was noticed that the patient had stopped breathing; that the face was livid; and that the pulse had ceased to beat. The face then rapidly became pallid; but subsequently resumed much of its duskiness. These symptoms supervened suddenly, without stertor or any other warning. Artificial respiration, laryngotomy, &c., failed to revive him.

At the necropsy, twenty hours after death, there was but slight rigor mortis. The head, face, and dorsal part of the trunk

were of a purplish colour. The parietes of the chest and abdomen were heavily laden with a thick layer of fat; the omentum was in a similar condition. The heart was rather large; its consistence flabby, and its valvular apparatus healthy. Externally, it was laden with fat; and, on section, the muscular substance appeared to have undergone slight fatty changes. Its right side and the large venous trunks were gorged with dark semicoagulated blood. Its left side was somewhat contracted, and nearly empty. The lungs were congested, but otherwise healthy. The kidneys were rather large; their capsules separated readily; they were congested, and presented slight fatty changes. There was subarachnoid effusion over the vertex of the brain; and the lateral ventricles were full of clear straw-coloured fluid. The other organs and the soft parts of the body generally were congested.

Case 2.—A woman, aged 43, the subject of orthopnœa, due to aneurism of the aortic arch of some standing. A firm pulsating tumour, of the size of the large end of an egg, presented in the middle line above the top of the sternum. She had great difficulty in breathing; and marked œdema of the left side of the face. She was decidedly syphilitic. After undergoing treatment by rest in bed, low diet, and iodide of potassium for a month, without any improvement taking place, it was decided to ligature the left common carotid artery. When she appeared in the operation-theatre for this purpose, she was in a very weak state; an attempt to place her in the recumbent position brought on a severe attack of dyspnœa. She declared she never could endure this position, and that it would kill her. Being propped up by some pillows, however, she was persuaded to recline somewhat; and chloroform was then administered on lint, as in the preceding case. She inhaled it quietly for a few minutes; then she turned pale; ceased breathing; became cyanotic, and died. This happened before the operation had been commenced. Such remedies as artificial respiration, inflation, and laryngotomy were resorted to; but in vain.

At the necropsy, twenty-four hours after death, a remarkable complication of disease was revealed. The face, neck, and fingers were of a purplish colour, and rigor mortis was present. The superficial veins of the neck were enormously swollen, and full of dark blood. The deep veins of this part were similarly affected; but the left internal jugular was shrunken and almost empty. A sacculated aneurism, of pyriform shape, measuring three inches in its long diameter, was found springing by its base from the transverse part of the aortic arch, with which it communicated by a round orifice of the size of a florin. It passed upwards, between the innominate (which was dilated) and the left common carotid arteries, to the fossa at the top of the

sternum, where its apex had been detected during life. The wall of this aneurism was very thin in front, where it adhered to the back of the first piece of the sternum, having compressed and completely obliterated the left innominate vein at this spot, Elsewhere it was thick, and adherent to the abundant deposit of laminated fibrin, which occupied nearly two-thirds of its interior. Behind, it touched the trachea, which was slightly flattened at the point of contact, but not to such an extent as to interfere with its patency. The adjacent nerves and other important structures had sustained no damage from the aneurism. The right side of the heart was distended with dark semicoagulated blood; the left side was flaccid and empty. The auriculo-ventricular valves were beaded with old calcarous and atheromatous nodules; the aortic valves, in addition to being similarly affected, were hypertrophied and dilated. The organ was large and hypertrophied, especially the left ventricle. Its muscular substance appeared to be healthy. On both sides, the lungs were firmly bound to the chest walls, throughout their whole extent, by old adhesions. They were large, emphysematous, and intensely congested. On section, abundant muco-purulent matter exuded from the divided bronchi. The brain was much congested; and there was excess of serous fluid in the lateral ventricles. The other organs presented no noteworthy changes.

Case 3.—I will now relate the case of a woman, aged 55, who was admitted with a strangulated femoral hernia and stercoraceous vomiting. The symptoms of strangulation were of fifty hours' duration. She was brought into the operation-theatre in an extremely weak and exhausted state. Anæsthesia was induced by nitrous oxide gas and then it was kept up by ether, Clover's apparatus being employed. She was very quiet from the commencement of the process; and there was no muscular excitement. Sufficient anæsthesia was thus induced in four minutes. Gentle taxis was then applied to the rupture. After the first few manipulations, the patient suddenly became pale: she vomited grumous stercoraceous matter; the pulse ceased; the breathing became weak, and she soon died. In this case, after other remedies had been tried, a weak solution of sal volatile was injected into the right median basilic vein; but without producing any obvious effect.

The necropsy took place twenty-eight hours after death; when rigor mortis was well marked. The trachea and larger bronchi contained some of the stercoraceous vomit; but none of it could be traced into the smaller divisions of the air passages. The lungs were slightly congested, but otherwise remarkably healthy. The heart was flabby and dilated, its walls thin, but not otherwise diseased. The right side con-

tained dark fluid blood, and the large veins were rather full; the left side was flaccid and almost empty. The valvular apparatus was normal. The aortic arch presented numerous patches of atheroma. The peritoneum was injected, especially in the vicinity of the damaged gut, where there was an effusion of recent lymph. The stomach contained much grumous fluid, and a large quantity of partially digested food, among which large pieces of cabbage and potato could easily be distinguished. The kidneys, liver, and spleen were normal. The uterus presented several small fibroid growths. The strangled gut comprised about four inches of the lower part of the ileum. It was of a dark purple colour, but not much thickened, and still retained its glistening appearance. There was no rupture of its walls. The brain was not examined.

Case 4.—This patient was a woman, 52 years old. She had a round-celled sarcoma of the left upper jaw, the size of a small apple, which was of nine months' growth. She was pale, thin, sallow and weak. It was proposed to excise the left upper jaw bone, and the growth with it. The anæsthetic agent used was the mixture of chloroform and ether. It was given in a leather inhaler, perforated at the top with small holes, and containing a sponge inside. The patient's head and shoulders were first propped up with pillows, etc. After the anæsthetic had been inhaled for a few minutes, and before she was well under its influence, the operation was commenced. She struggled, and seemed to feel the pain. Owing to the nature of the subsequent proceedings, but little of it could have been inhaled afterwards. As the bone was being wrenched away with the lion forceps, she suddenly ceased struggling and became pale. The removal was quickly completed, and artificial respiration was resorted to. The throat was examined for a foreign body, but none could be found. As considerable bleeding occurred it was frequently mopped out with sponges on holders. She never showed the slightest sign of revival.

At the necropsy, twenty-six hours after death, there was well marked rigor mortis; and the head, neck, and upper part of the trunk were congested. The trachea and bronchi contained a little semicoagulated blood, loosely adherent to their walls; and blood could be traced, at some parts, even into the smaller bronchi. The lungs were healthy, except that their bases were congested behind. For such a thin woman, there was an unusually large amount of fat about the outside of the heart. To the naked eye, its muscular substance appeared healthy. The left ventricle was nearly empty; the right moderately full. In other respects, the heart was normal. The liver contained some hydatid cysts. The other organs presented no noteworthy changes.

Case 5.—As in the three preceding cases, this patient was a woman. Her age was fifty-three years, and she had suffered from cancer of the womb for the last eighteen months. Three weeks ago subacute symptoms of intestinal obstruction came on, owing, as was believed, to adhesions between the gut and the womb. During the week preceding the present crisis, she had no action of the bowels; fæculent vomiting was frequent; and she was unable to retain any food. Enemata, etc., gave no relief; therefore, it was decided to open the colon in the left loin. She was a large, pale, emaciated, and feeble person. The anæsthetic employed, and the mode of its administration, were precisely the same as in the last case. She died suddenly before the process was complete, and before the operation had been commenced.

At the necropsy, twenty-four hours after death, muscular rigidity was fairly marked. The heart was quite free from disease. The right ventricle contained a gelatinous clot, and the left auricle some fluid blood. Some of the fæculent vomit was contained in the air-passages, and could be traced into the smaller bronchi in the upper and middle lobes of the right lung; the base of the left lung was deeply congested behind. The œsophagus was full of this fæculent material. The parietes of the abdomen were enormously distended, owing, as it proved on dividing them, to the fulness of the intestines. There were signs of comparatively recent general peritonitis. The omentum was adherent to the fundus of the uterus, and tightly stretched over the distended intestines. This distension was chiefly limited to the small intestines, which were full of fluid fæculent matter; the cause of it was an obstruction of the bowel, at about six inches above the ileo-cæcal valve, where it was adherent to the uterus, only a very small passage still remaining open. A second obstruction effectually preventing the passage of solid fæcal matter, existed in the sigmoid flexure of the colon, which was also adherent to the uterus. The transverse colon was distended with flatus. With the exception of the fundus, almost the whole of the uterus had been destroyed by cancerous ulceration; and its place was occupied by a sloughing cavity, communicating with the vagina. The right kidney weighed only two and a quarter ounces; it was small and sacculated; very little of its secreting structure remained, and its ureter was dilated. The left kidney was normal; and so were the other organs examined.

Case 6.—The last case I have to relate is that of a farm-labourer, aged 44, who had cancer of the right side of the tongue, encroaching somewhat on the soft palate. The disease had invaded the glands of the neck, especially those of the right side; and it was of nine months' duration. It had pre-

viously been excised, and subsequently destroyed by the galvano-cautery—only to return again, however. On account of the advanced nature of the disease and the impossibility of completely extirpating it, further operative treatment was not advised; but, subsequently, at the patient's urgent request, it was decided to remove the tongue and as much of the disease as could be got at. His general health was fairly good, though he was somewhat emaciated. Having been propped up, in the position usual for such operations, the mixture of chloroform and ether was administered, exactly as in the two preceding cases. Only partial anæsthesia was induced, with a view of obviating the ill effect of anticipated hemorrhage. A gag was then introduced into the mouth, and the right cheek was slit freely open. The wire loop of the *écraseur* was next passed round the base of the tongue, behind the curved-handled needles previously introduced. As soon as the *écraseur* was tightened up, the patient became pale, and ceased breathing. A few hasty turns of the screw sufficed to complete the removal of the tongue. The throat was mopped out with sponges, and examined for a foreign body, but none could be detected. Artificial respiration, tracheotomy, and galvanism were resorted to; but the patient could not be revived.

At the necropsy on the following day, rigor mortis was present, and the integuments were pallid. The lungs were firmly bound to the chest-walls by old adhesions. They were emphysematous and congested at their bases, but crepitant throughout. The trachea and larger bronchi contained some blood and mucus; and blood could be traced into the smaller bronchi, especially in the lower lobes. The right side of the heart contained a large quantity of dark semi-clotted blood, and the left side contained some fluid blood. The right ventricle was dilated; the left contracted. The valvular apparatus and the muscular substance were normal in every respect. The kidneys were large and congested, but otherwise healthy. There was nothing abnormal about the other organs examined.

I have observed that those who administer anæsthetics too often, do so without any fixed principles to guide them. This is lamentable, because, as many of these cases show, the fundamental laws of the anæsthetic art cannot be disregarded without entailing a deplorable sacrifice of life. I will here endeavour to state, in the briefest manner possible, what I consider to be the most important practical inferences to be drawn from them.

With regard to chloroform, then, subject to the attainment of the object in view, too much air cannot be given during its administration; and with regard to ether, too little air cannot be given during its administration. From this it follows, that

a long time is required to induce anæsthesia by chloroform; but to produce the same result with ether, a short time is sufficient. Now, by a long time, I mean about a quarter of an hour, and by a short time, about five minutes.

Surgeons are not unfrequently to blame in this respect. How often one has heard it said to the chloroformist—"Be as quick as you can; I want to begin the operation in *five minutes*." In my opinion, this is equivalent to saying—"Kill at least 1 per cent. of my patients." Those inhalers are the best which most readily facilitate the fulfilment of these requirements. For giving chloroform, one with a wire framework, having a diaphragm of flannel, or some similar material, stretched over the top of it, on which to evaporate the anæsthetic, but open at the sides, would be very good; but a piece of lint, or the corner of a towel, properly used, would do as well. A graduated drop-bottle is necessary in any case, as only a small quantity of chloroform should be poured on at a time, and this requires to be frequently renewed. For the administration of ether, Ormsby's inhaler seems to me the best; it was designed to fulfil the requirements just mentioned, and I have found it answer admirably.

There is only one point I will now mention, and that is the importance of watching the respirations during the process. To do so properly, of course the epigastrium must be uncovered. It is of much greater value than feeling the pulse, since, when the latter stops, there, as a rule, is an end of the patient. Mr. Lister has very ably insisted on this. However, I have found it generally neglected at King's College Hospital. — *British Medical Journal, Feb.* 24, 1883, *p.* 350.

88.—ON THE SAFE ADMINISTRATION OF CHLOROFORM.

By William Martin Coates, F.R.C.S., Surgeon to the Salisbury Infirmary.

In the year 1858 I made known to the profession in a *brochure* my views on the "Safe Administration of Chloroform." This little work was most favourably reviewed by the medical press, and several surgeons more or less eminent wrote to me their conviction that I had solved the problem of greatly limiting, if not abolishing, the dangers of this anæsthetic. By experiments on frogs and observations on patients, I had become convinced that chloroform could only be safely administered by limiting the dose to the smallest quantity capable of inducing insensibility to pain. By repeated trials I found that by means of Snow's inhaler five minims of this anæsthetic, followed by ten in twenty seconds, and in forty seconds by fifteen, and then fifteen every minute until the patient became insensible, and

afterwards an occasional ten minims, sufficed in almost every case to produce and maintain complete anæsthesia. Very rarely twenty minims were required. It was found that when fifteen minims were put into the inhaler every minute during the inhalation all the chloroform had evaporated at the end of that time. I have reason to think from the publication of these facts the doses were much moderated, and that in consequence the number of deaths became relatively less. Although I have during these twenty-four years never been prevented administering it by extreme age or infancy, by chronically diseased heart, lungs, or kidneys, I have not had a death by chloroform. Considering that I have been surgeon to the Salisbury Infirmary during all these years, and have had during that period a numerous *clientèle*, this evidence will not, I trust, be considered unimportant. I have read with painful interest the reports of deaths from chloroform, and have not come across one in which this mode of giving it had been adopted. In every case of death in which the quantity of the anæsthetic inhaled was recorded, it was much larger than that advocated by me. Of late years a quicker and more daring plan has been advised and practised. Hence, I feel sure, the more frequent fatal results. During these twenty-four years I have never refused chloroform to any patient in whose case pain was anticipated. The scares inducing pressure of the chest, placing the head below the level of the body, &c., have not occurred. Sometimes those who have kindly administered the chloroform have preferred the towel and handkerchief. This I consider dangerous, and now always request that the plan detailed above be adopted. By it the sickness, headache, and malaise, following operations performed under chloroform, are immensely diminished. Some timid patients fancy that the mouthpiece of the apparatus will suffocate them. In such cases I sprinkle ten minims of the anæsthetic on a handkerchief, and when it begins to act return to the inhaler. Mr. Matthews has supplied me with three sizes of mouthpieces, surrounded by inflated india-rubber tubes and more perfect valves. These are much better than those used by Dr. Snow. When the towel or handkerchief is used the dose must be more than doubled, as more than half is lost by evaporation and expiration; but then the atmosphere becomes loaded with vapour, and if danger should arise the chances of life to the patient would be much diminished. It would seem almost unnecessary to give facts or arguments to prove my position, for what would any educated practitioner think of one who having made up his mind to give a grain of opium every hour for twelve hours gave twelve grains at once? or having decided to give twenty grains of chloral every four hours for twelve hours administered sixty grains at one dose?

I admit that these are extreme illustrations, but the principle is the same. We all know that some patients are prone to be rapidly and profoundly affected by mercury, opium, and chloral hydrate, &c. I am certain that some, and not a few, are dangerously affected by the usual doses of chloroform. Curiously, I have had several illustrations of this in the wards of the Salisbury Infirmary lately. One young woman of twenty-four years of age was completely narcotised by five minims of chloroform. A middle-aged woman was rendered insensible to pain during an operation lasting a quarter of an hour by seventy-five minims, and a child by ten minims. Had the usual doses been given to these patients, would not their lives have been placed in danger? I think so. I admit that in most cases by the withdrawal of the anæsthetic, opening the windows (which during the operation should never be shut), &c., the patient is mostly saved, and the fatal cases reduced to one in a few thousands, but to that one this makes all the difference between life and death.

It has been said above that I have not in one instance refused to give chloroform because of diseased heart, feeling that indications of danger would declare themselves in time to combat them. Two illustrative cases have occurred in private practice during the last few weeks. One was a gentleman seventy-nine years of age, on whom I performed colotomy. His pulse was very feeble and intermittent. The other case was an elderly lady with tricuspid insufficiency very marked. These cases have an additional interest from my having made an important addition to my usual method of giving the anæsthetic. It struck me that it might be useful in cases in which the heart showed symptoms of powerlessness to give a dose of pure ether by inhalation in the same relatively small quantities with the view of rallying this organ. These two cases prove, I think, that this idea is a valuable one. The old gentleman was given chloroform after my method, and when the pulse dropped at all twenty minims of ether were put into the modified Snow's inhaler. The pulse on each occasion answered to the whip instantly, and was firmer and more regular at the end of the operation than at its beginning. The elderly lady suffered from hemorrhoids at both sides of the anus, and from a very painful fissure. She was very anæmic, and her skin was of the colour so familiar to us in the case of malignant disease. We could, however, detect none. A gentleman who had had much experience in giving chloroform with a towel or handkerchief, declined, in consequence of the tricuspid disease and extreme anæmia, to be the administrator. As this case was more than usually risky, I requested Mr. Harcourt Coates to operate, I

administering the anæsthetic. The patient objected to the inhaler. Ten minims of chloroform were dropped on a handkerchief which was placed over her mouth and nose. She soon became sufficiently unconscious to admit of the use of the inhaler. The operation lasted from first to last half an hour. She was kept insensible to the pain by seventy-five minims of chloroform and the same quantity of ether. Her pulse varied but little during the half hour. When it became a little depressed, twenty minims of ether raised it instantly. In no case since the year 1858 have I had to use galvanism, nitrite of amyl, artificial respiration, or any other mode of resuscitation.

My readers have, no doubt, compared the small doses of ether with those habitually given. May it not be that the same freedom from danger from this anæsthetic would be attained if the same quantity only was inhaled? Before I publish my contemplated next edition of the little work on "Chloroform and its Safe Administration," I hope to work out this practically, both as regards ether and the bichloride of methylene. The economy of chloroform is necessarily very great by this method. I admit this to be of least consequence. Am I rash in concluding that the foregoing facts are calculated to prove:—

1st. That the plan advocated is rational and consistent with our practice with other powerful remedial agents.

2nd. That it lessens the dangers and after disagreeable effects of the anæsthetic, and so would enlarge the area of its utility?

If so, I am very thankful; as the frequent deaths lately reported, should they continue to occur, would naturally increase the anxieties both of the public and of ourselves, and might limit to a fearful degree the blessing of that merciful gift of God, anæsthesia.—*Lancet*, *Dec.* 23, 1882, *p.* 1070.

89.—ON THE PROPRIETY OF INVERSION OF PATIENTS IN THE CHLOROFORM SYNCOPE.

By Eben. Watson, M.D., Senior Surgeon to the Glasgow Royal Infirmary.

Syncope, or sudden failure of the heart's action, is the chief source of danger to the patient, and of anxiety and difficulty to the surgeon, during the administration of chloroform. It is well known that this may take place before suspension of the respiration, or the two phenomena may occur almost simultaneously, the latter being their more frequent relation to one another.

Now, I wish very briefly to call attention to the actual state of the heart and lungs in the above condition of the

patient, a condition which, in short, is syncopal asphyxia. In this state we have long had abundant evidence to prove that the right side of the heart is full of blood, and that the lungs are congested. I shall not here inquire which event is the primary one. It could easily be proved that the two events are differently related in different cases, but that in all cases the one increases the other. A feeble heart does not send the blood with sufficient force through the lungs, and so permits congestion to take place in them; while the lungs, if congested from any cause, resist the influx of blood from the heart, and therefore cause it to be gorged on the right side; and this engorgement, again, greatly impedes and enfeebles the action of the heart. As might *a priori* have been expected, the same condition of the heart and lungs has been found by all experimenters with chloroform. Thus Professor M'Kendrick and his coadjutors, in their report to the British Medical Association (p. 30), writes as follows: "One of the most striking effects of anæsthetic agents is the engorgement of the right side of the heart and large veins near it. This has been directly observed by the committee, and is well known." They then proceed to discuss the share taken in this engorgement by the heart itself, which is known to be enfeebled by anæsthetics, especially chloroform, and by certain changes in the lungs which they found to be much congested in all cases of deep chloroformisation. Into this discussion we need not at present follow them; I only wish to direct attention to the fact that in a state of deep anæsthesia from inhalation of chloroform there are always coexistent (1) a feebly acting heart, (2) an engorged state of the right side of the heart, and (3) a congested state of the lungs. In the suddenly alarming cases where syncope occurs it cannot be doubted that all these conditions are exaggerated in an extreme degree, and the object of treatment certainly ought to be the restoration of the organs to a normal state as speedily as possible.

Now in all writings and text-books on this subject one of the recommendations to the surgeon, in order to accomplish this end, is to invert the body of the patient by raising his lower extremities above the level of the head and the upper part of the body. This practice is said to have had its origin in an experiment performed by Nélaton, and it generally goes by his name. His great character as a surgeon has given it a wide currency, a currency which, so far as I know, was not warranted by either the accuracy or the conclusiveness of his experiment. It is now blindly followed, I think, without consideration of the effect of the proceeding, or with very loose notions of its effect. Thus, in an otherwise good paper by Mr. Samuel Osborn, chloroformist to St. Thomas's Hospital, I find these

words (p. 15):—"Inversion of the body should be always tried if stoppage of the heart's action occur. The head being lowered and the legs elevated, the blood is sent to the upper part of the body." Of course it can only be the blood in the veins of the lower part which can be so passed to the upper part of the body, and everyone knows that it must pass first through the right side of the heart, and then through the lungs, before it can be sent to the upper part of the body. But the evil against which we are supposed to be contending is that there is too much blood already in the right side of the heart and lungs, and that it cannot get on. To send more venous blood to these organs just then is surely to aggravate the mischief, especially where the heart's force is greatly diminished, and the respiration is inefficiently performed. Even although the latter function be carried on artificially it cannot be sufficient of itself to renew the pulmonary circulation while the heart remains feeble, and one great cause of its continuing feeble is the weight and paralysing influence of engorgement of its right cavities. It seems to me, then, that this inversion of the body has been recommended in these cases without due consideration, and that it is not suitable to them. If, in spite of the valves in the veins, the venous blood from the neck and arms did get to the brain by depressing the head and neck, as is sometimes done, it could only deepen the coma and increase the evil from the side of the nerve centres; and, as I have shown above, the venous blood from the lower part of the body, if forced upon that already filling the right side of the heart and lungs, could only embarrass the circulation, and still farther add to the danger of the patient.

The best position for such patients, as for all in syncope, is the horizontal position, because in it a feeble heart has least hindrance to its work in sending, not venous blood to the lungs, but arterial blood, both to its own substance and to the brain. No doubt these two results of cardiac action take place simultaneously, but it is on the latter—viz., the sending forth of arterial blood, that our hopes for the patient mainly depend, and the importance of artificial respiration in assisting this is obvious to all who rightly consider the matter. The *horizontal position*, along with artificial respiration, is that which favours most the reinforcement of the heart, and this is the one object of hope in all such cases. But the practice of inversion of the body has been so constantly recommended that no one at present thinks he does his duty to his patient unless he has recourse to it, and I believe some operating tables are now made with a lever so as to enable the surgeon readily to invert the table and the patient upon it. Now this, I submit, is both an unnecessary complication of the table and a practice really hurtful to the

patient, because in so far as it succeeds in sending venous blood of the lower part of the body to the heart it tends to embarrass and weaken that organ, already labouring under influences formerly explained, against which the best remedy actually is to diminish the blood, which stagnates in the right heart and lungs; and this we attempt to do by artificial respiration with the patient in the horizontal position more physiologically, and therefore more hopefully, than by inversion.

I have no doubt on several occasions seen inversion performed in cases where, during chloroformisation, there was a temporary cessation of the pulse or the respiration or both—in fact, where syncope had occurred and in which recovery took place; but I believe the recovery was in spite of, rather than because of, the inversion of the patient. In fully as many cases of the kind I have not followed this practice, and yet the recovery was as rapid and as complete as in the others. It is in no spirit of boasting, but rather of thankfulness, that I state, notwithstanding my long experience of chloroform, now extending to more than twenty years of hospital and private practice, I have never yet seen a patient die of that anæsthetic.

In all cases where I have been present, the result has been uniformly fortunate, whether Nélaton's method was used or not, and therefore it may perhaps be said that my experience proves nothing one way or other; but undoubtedly there have been a good many, only too many, unfortunate cases, both recorded and unrecorded, in which the patients died in the chloroform syncope, and I believe in almost all of them inversion was performed, but did no good; and, if my view of the matter be correct, it rather did harm. The surgeons in these cases certainly did their duty in performing inversion, because it is at present the recognised practice; but now, when it is asserted to be contrary to sound views of the physiology and pathology of the case, it may cease to be any longer recommended or practised. At all events, it will surely come to be more accurately discussed, and either established on a sound basis, if that be possible, or authoritatively discarded altogether, as I think it ought to be. With this object I now write, and appeal to my surgical brethren, especially to those of them who can look at the matter from the side of physiology. If I am convinced that my opinion is wrong, I shall be very ready to confess it; but if I am right, I may have assisted in simplifying the treatment of some very alarming cases, in which one would wish to do everything that is possible towards their recovery.—*Lancet*, *March* 10, 1883, *p.* 401.

90.—ON DIALYSED IRON, AND ITS THERAPEUTIC USE.

By Prosser James, M.D., Lecturer on Materia Medica and Therapeutics at the London Hospital, &c.

Dialysed iron, in a form adapted for medical use, has now been long enough before the profession to allow us to form an estimate of its position among ferruginous preparations. Of these, the per-salts of iron are so astringent that they are frequently employed for this property only; while the proto-salts are so much less astringent that we occasionally hear them spoken of, rather inaccurately, as destitute of this quality. Freshly prepared carbonate is a very valuable preparation when a mild chalybeate is required, but some objections to it lead us, in many cases, to prefer the *ferrum redactum*. This last, however, is not always well borne by the stomach. The elegant scale preparations of the salts with vegetable acids have been popular on account of their solubility and slight astringency, while to most persons they are less disagreeable to take than the other salts. Lately, when all these forms disagree or seem unsuitable, we have had at our disposal a still milder chalybeate in dialysed iron, which is not unpleasant to the taste, and does not irritate the stomach or bowels.

For therapeutic use when the astringent properties are indicated, the per-salts should be employed; but when astringency is not required, it is better to select the proto-salts and other milder preparations, according to their relative strength and liability to produce disagreeable symptoms. When small doses of the vegetable salts or of reduced iron are not well tolerated we may rely upon dialysed iron. Inverting a common practice, would it not be as well to make a rule of giving the least irritant preparation of all whenever there are no indications for the stronger ones? As dialysed iron does not interfere with the digestive organs, it may often be given when no other chalybeate is tolerated, and it is not necessary to precede its use by the time-honoured aperient, nor to repeat that dose at more or less frequent intervals. It will, of course, be understood that in making this statement I presume that iron is indicated, and that the digestive organs are not in such a condition as to forbid it.

The effect of chalybeates is generally measured by clinical observation. The testimony of physicians to the activity of dialysed iron is so abundant that it is unnecessary to add to it. That the metal is readily taken into the blood is not to be doubted, although some have supposed that there would be a difficulty in the absorption of particles which do not pass through the dialysing membrane. But this suggestion can have no weight, considering the numerous insoluble substances

which are at once so changed in the stomach as to become easily assimilated. Moreover, the hæmatinic effects of the remedy may be measured by the modern method of counting the blood-corpuscles; and Dr. Armory has succeeded in projecting upon a screen a magnified image of the slide employed in this method. In five caaes of anæmia with diminished corpuscles, as determined by the hæmacytometer, the globules rapidly increased under the use of ninety drops daily, and general improvement went on *pari passu* wfth this increase. Further observations will, no doubt, confirm these results, and the estimation of the state of the blood by enumerating the globules will continue to be of the greatest use. It is true that, according to Hayem, the number is not always affected by iron; and C. Bernard found that the corpuscles were not invariably deficient in the blood of chlorotic patients. On the other hand, Hayem found the globules improve in quality, size, colour, etc., under the influence of iron in cases in which there was no deficiency in their number.

The average dose of dialysed iron is from twenty to fifty drops daily, in three doses. Dr. Weir Mitchell gives a drachm of the solution at a time, and finds that such doses neither "constipate nor affect injuriously the process of digestion." I have sometimes followed him in the use of these doses, but usually prescribe from ten to twenty drops after every meal. The dose may be taken in a little water or on a lump of sugar. Professor Da Costa has used it hypodermically with great benefit, beginning with fifteen minims, and soon increasing to thirty. No inconvenience, local or general, was produced, and he suggests this method of administration in gastric ulcer, pernicious anæmia, and when, from defective absorption or assimilation, it seems desirable to introduce the remedy directly into the system.

As an antidote to arsenic, dialysed iron deserves attention. The rapidity with which it acts on arsenical preparations is easily observed in a test-tube, and naturally suggests it as a substitute for the moist peroxide. In the Philadelphia Medical Times for December 8, 1877, Dr. T. B. Reed recorded a case of poisoning by arsenic in which he had very successfully employed dialysed iron; and quite a number of cases have since been published. The solution has the advantage of being ready when wanted, while the older antidote has to be freshly prepared for the occasion—a process necessarily involving loss of time.—*Medical Times and Gazette, Dec.* 2, 1882, *p.* 659.

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